

# THE AMERICAN NATURALIST

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Vol. XLIV

July, 1910

No. 523

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## A CONSIDERATION OF THE "SPECIES PLANTARUM" OF LINNÆUS AS A BASIS FOR THE STARTING-POINT OF THE NOMENCLATURE OF CRYPTOGRAMS<sup>1</sup>

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At the Congress held in Vienna in 1905 it was voted to adopt Linnaeus's "Species Plantarum," 1753, as the starting-point of the nomenclature of flowering plants and the question of the starting-point for that of cryptogams was referred to the congress to be held at Brussels in May, 1910. The adoption of the "Species Plantarum" was endorsed practically by so large a proportion of phænogamic botanists that its acceptance came as near being universal as could ever be expected in such a case. It may be assumed therefore that the "Species Plantarum" is well adapted to serve as a basis for the nomenclature of phænogams. Were it true that it is as well, or nearly as well, adapted to serve as a basis for the nomenclature of cryptogams, there would be no hesitation on the part of cryptogamists in adopting it also. If it is not, there is no reason why they should feel under any obligation, for the sake of a merely formal uniformity in nomenclature, to follow in the steps of other botanists.

In the first place we may ask why it is that the "Species

<sup>1</sup> Invitation papers read at the sixteenth annual meeting of the Botanical Society of America.

Plantarum" should be considered to be well adapted to the requirements of phænogamic botanists. The fact that it was the first work in which the binomial nomenclature was methodically applied is a sufficient reason why no work issued prior to 1753 should have been adopted as a basis of nomenclature, but that fact alone is not a sufficient reason for the adoption of the "Species Plantarum" itself. An examination of that work shows also other merits which should recommend it. It is an admirable summary by the leading systematist of his day of several hundred genera and some thousands of species found not only in Europe but also in North America and other more remote parts of the world. In fact, on glancing over its pages one is surprised at the large field covered by Linnæus and the large number of exotic species which are included in the work. In the numerous editions of the "Species" issued at intervals of a few years until as late as 1830, some under the title of "Systema Vegetabilium" and "Systema Plantarum," the Linnæan traditions were handed down with additions and annotations by well-known botanists, so that there is no gap separating the original edition from the date of the appearance of the first volume of De Candolle's "Prodromus" in 1824. It should also be borne in mind that under the careful guardianship of the Linnæan Society of London, the Linnæan herbarium is still in existence and accessible to botanists. It is therefore not difficult to see that for a basis of nomenclature of flowering plants the "Species Plantarum" was well chosen.

If we turn now to the cryptogams of the "Species" we find a very different state of things. To those who have not examined the "Species" with reference to this point it might seem that the cryptogamists for the sake of uniformity might be willing to make some sacrifice. For such persons a comparative examination of the phænogams and cryptogams in the "Species" may be of interest. For this purpose I have prepared a table showing the number of genera and species in the two groups.

The number of genera can be determined without difficulty. The counting of the species is less easy since in some cases it is not quite certain whether under a given name Linnæus intended to indicate a species properly speaking or merely a form or variety. In my enumeration I have included only those forms clearly designated as species, omitting subspecific forms. That the enumeration here given is conservative is shown by the fact that, while according to the "Codex Linnæanus" the total number of species in the "Species Plantarum" is 5,938, the total of my list is 5,247, divided as follows:

	Genera.	Species.
Phænogams.....	1,049	4,630
Cryptogams.....	50	617
Total.....	1,099	5,247

Of the 50 genera and 617 species, 16 genera and 189 species are Filices and there are in addition 24 species of the genus *Lycopodium* which was placed by Linnæus in Musci. Among the Filices are to be found numerous characteristic species of America and the tropics and in this respect the treatment of the group by Linnæus is quite comparable with his treatment of phænogams. For nomenclatorial purposes the Filices and *Lycopodium* are even at the present day treated in the same manner as phænogams, and it is a well-known fact that it is the custom to unite the vascular cryptogams and the phænogams in floristic works. So far as we are now concerned the higher cryptogams need not enter into the discussion, but from the nomenclatorial standpoint must be classed with phænogams and there are therefore left 404 species and 44 genera for all the Bryophytes and Thallophytes described in the "Species Plantarum." But even in this small number is included the genus *Spongia* under Algæ with 11 species, of which at least the greater part are not even plants in any sense. Furthermore, among the Bryophytes and Thallophytes there are almost no extra-European species and of the European species a great

proportion are northern. In short, although as far as phænogams are concerned the "Species Plantarum" includes characteristic representatives of different parts of the world, as far as the Bryophytes and Thallophytes are concerned it represents only a limited European flora.

The question may perhaps be asked by those who have not studied specially any group of non-vascular cryptogams: Although the number of non-vascular cryptogams in the "Species Plantarum" is very much smaller than that of phænogams, is it not perhaps the case that the ratio represents approximately the relative size of the two groups in nature? It has been the custom to state that the phænogams outnumber the cryptogams, some even saying that they are much more numerous. Such statements are based solely on an enumeration of described species and fail to give information as to the probable actual number of species. It is not possible to give figures on the subject which are up to date and the statistics of even a few years ago are of slight value, for it is only within a few years that the study of cryptogams has been pursued in other parts of the world than Europe or, to a less extent, North America. We can probably obtain a more correct opinion if we consider probabilities. The number of known species of Musci and Hepaticæ has been very much increased in the last few years, and although the bryological flora of Europe and North America is now so well known that no very large number of new species is to be expected there, in other parts of the world and especially in the tropics, it is evident that the work of exploration conducted by trained specialists will bring to light a very large number of new species. The same is true of lichens. In algæ a very great increase of species is less to be expected, partly for the reason that the region of growth of marine algæ, pelagic species excepted, is more limited than that of land plants. But even in algæ, it is probable that the known species will be considerably increased.



The fungi offer a better field for comparison than other groups. It is certainly true that the number of described species is decidedly smaller than that of phænogams. Are we then to conclude that there are fewer fungi in the world than there are phænogams? By no means, for there is a possible inference which may be drawn from a knowledge of the distribution of fungi to which, it seems to me, great weight should be given. Year by year the number of known parasitic fungi goes on increasing and, although we can not assume that probably every phænogam has its parasite, the proportion which have is constantly increasing. We also know that some species have not only one but many parasites and, as a rule, the species which from their economic value have been most carefully studied are the hosts of many fungi. As an instance I may mention the species of the genus *Vitis*, on which several hundred species of fungi are known to grow, some, to be sure, found also on other plants, but a large number peculiar to this genus. When all genera have been studied as carefully as *Vitis* we shall undoubtedly find that the number of parasitic fungi in existence is enormous. If to the parasitic we add the thousands of saprophytic fungi, it may well be asked whether eventually it will not prove to be true that the number of species of fungi is as great as that of phænogams. It seems to me that it should be plain to every one that if in the "Species Plantarum" the proportion of phænogams to cryptogams is about ten to one, we must admit that although the work is sufficiently comprehensive to serve as a basis for the nomenclature of the former, it is entirely inadequate in the case of the latter.

I have referred to the restricted range of the species of cryptogams described by Linnaeus and to their small number. If we go farther and examine the character of the descriptions themselves we find that they are in many cases vague and unintelligible, which is nothing more than might have been expected in that day before the scientific study of the group had really begun. The Algae

in particular are from the modern point of view a strange medley. The genera *Jungermannia*, *Targionia*, *Marchantia*, *Blasia*, *Riccia* and *Anthoceros* I have in my enumeration included in Bryophytes where they properly belong, although they were placed by Linnæus in Algæ as well as the genus *Lichen* with 80 species. The genus *Tremella* with 7 species was also included in Algæ, although as far as the scanty descriptions can be identified, 3 are species of fungi, 3 algæ and one a lichen. Some of the 12 species of *Byssus* are algæ, but the majority it is impossible to recognize. Of the 11 species of *Spongia* nearly all are animals.

Of the later editions of the "Species Plantarum" the fourth, according to some the fifth, has a partial revision of the fungi by Link and of the mosses by Schwaegrichen, but as these parts were not published until 1824-30 and do not follow in any way the original edition of Linnæus, so far as priority of nomenclature is concerned, they need not be considered here. Of the "Systema Plantarum," Reichard, 1780, and the "Systema Vegetabilium" by Gmelin, 1796, by Persoon, 1797, and Sprengel, 1827, it can be said that although they include more species than the original edition of Linnæus they are open to the same objection and, as will be seen later, the dates of their publication are so near those of far better works that their nomenclatorial value is of trivial importance. If I have dwelt at what may seem too great length on a consideration of the value of the "Species Plantarum" as a basis of nomenclature, it has been for the purpose of trying to make clear to those to whom uniformity in nomenclature seems to be of the first importance, why it is that to expect cryptogamists to adopt the "Species" on the same basis as do phænogamists is unreasonable. To the latter the "Species" represents a fundamental treatise; to the former a very meager and unsatisfactory list of plants belonging to groups of which, in the time of Linnæus, there was really no exact knowledge.

One would be glad to adopt as a basis of nomenclature some one work which bears the same relation to cryptogams as does the "*Species Plantarum*" to phænogams, but there has never been any such work and there never will be, for a very good reason. The phænogams form a homogeneous group. The cryptogams do not, but consist of a number of different groups, and the fundamental works relating to them appeared at different dates, all, however, considerably later than 1753. The specialists who study bryophytes, lichens, algæ and fungi are entirely justified in adopting different works as a basis of nomenclature. The question they should ask is: What was the first work on bryophytes, on lichens, on algæ, on fungi, in which those groups were scientifically and comprehensively treated?

It is not possible to enter at this time on a general consideration of this point. Although that part of Linnæus's "*Species*" which relates to bryophytes appears to have greater value than that which relates to thallophytes, since for one reason his citations of Dillenius's figures help one to understand to what plants the brief descriptions were applied, it must certainly be admitted that Hedwig's "*Species Muscorum*," of which the first volume appeared in 1801, is the fundamental work on mosses and that Hedwig, with whom the scientific study of mosses began, may be called the Linnæus of bryology. Acharius stands in the same relation to lichenology, and it is a question to be settled by lichenologists whether the "*Lichenographia Universalis*" of 1810 or the earlier "*Methodus*" is to be given the preference. For algæ, the "*Systema Algarum*" of C. A. Agardh has been suggested. It is, however, out of the question to refer more in detail to the groups just mentioned, but it will be sufficient if we consider the case of fungi somewhat more minutely, although the subject is perplexing and complicated even to those more particularly interested in this group and probably wearisome to others.

In the "*Species Plantarum*" 1,073 pages are given to

phænogams, 15 pages only are given to fungi, including *Agaricus* 27 species, *Boletus* 12, *Hydnum* 4, *Phallus* 2, *Clathrus* 3, *Elvela* 2, *Peziza* 8, *Clavaria* 8, *Lycoperdon* 9 and *Mucor* 11. To these must be added 3 of the species of *Tremella* placed by Linnæus in algæ, making 89 fungi in all. Of these not one is extra-European and only 8 are cited as growing in Italy or southern Europe. To consider that a work of such a limited scope should serve as a basis of nomenclature of a group whose species are numbered by thousands seems to me preposterous. All that we can say of the fungi in the "Species Plantarum" is that they show plainly that in 1753 next to nothing was known of that large group, and one may be pardoned for saying that in what Linnæus wrote about fungi he was not a Linnæus. We must search elsewhere for a fundamental work on the subject. In the later editions of the "Species" and the "Systema Vegetabilium," as I have said, the treatment of fungi is not in any way satisfactory, and it was not until about fifty years after the publication of the "Species" that there appeared anything which could be called a general and comprehensive work on the species of fungi. If mycologists were asked who exerted the greatest influence in placing systematic mycology on a firm basis they would say Elias Fries and the "Systema Mycologicum," of which the first volume appeared in 1821, had an influence in shaping the study as no other work has had. In saying this I do not wish in any way to underrate the value of the "Synopsis Methodica Fungorum" of Persoon, issued in 1801, but of the two I think that the "Systema" is the one which has had decidedly the greater influence in shaping the progress of descriptive mycology. In its three volumes together with the two volumes of the "Elenchus" which is a part of the "Systema," we find for the first time an account of the mycological flora of a considerable portion of the world rather than an account of certain orders of fungi, mainly of Europe. In the "Epicrisis" of 1836-38, the "Summer Vegetabilium Scandinaviæ," 1849, and the

"Hymenomycetes Europæi," 1874, we have important revisions and commentaries by Fries of his earlier work. The "Icones Selectæ Hymenomycetum" include 200 plates executed under his supervision of species which cannot well be studied by dried specimens alone. The herbarium of Fries is still at Upsala and the "Scleromyceti Leuciæ," a collection of 450 small parasitic species, is to be found in herbaria in Europe and this country and has been the subject of critical commentaries by several botanists. The fact that the volumes of the "Systema" did not appear in the same year does not appear to me to present a serious practical difficulty, as Volume I containing Hymenomycetes appeared in 1821 (except *Solenia*, *Cyphelia* and the Tremellinæ, in Vol. II, 1822), Volume II, section 1, with Discomycetes appeared in 1822, Volume II, section 2 (p. 275), with Gasteromycetes (Angiogasters) and Pyrenomycetes in 1823, Volume III, sectio prior with Gasteromycetes, Myxomycetes and Perisporiaceæ in 1829, and Vol. III, sectio posterior (p. 261), with Fungi Imperfecti in 1832.

The "Synopsis" of Persoon, although to be preferred to any previous work, is considerably less extensive in the number and range of the species given than the "Systema," the number in the latter being about two and a half times as great and, in general, the "Systema" presents a decidedly more modern way of treating the group. A fuller consideration of the comparative merits of the "Systema" is out of the question in this place as it would require more time than can be allowed and because the details are such that they could not be readily followed except by mycologists who have studied the question minutely. I have no right to encroach further on your patience and need only, in conclusion, repeat that the "Species Plantarum" is quite unfit to serve as a basis for the nomenclature of fungi, and that the "Systema" of Fries seems to me to be better adapted for the purpose than any other work. In any case, to go back earlier than the "Synopsis" of Persoon would only tend to perpetu-

ate the present uncertainty and confusion, and would open the door to those who, regarding nomenclature as an end in itself and not merely a means by which the necessary evil of naming plants can be reduced to a minimum, devote time and labor to the undesirable task of unearthing names which are at the best uncertain, at the sacrifice of names which have been in universal use for many years, and whose meaning is perfectly clear. To my mind the object should be, not to attempt to seek perfection in authority and priority—a hopeless task—but rather to select the best solid basis in some comprehensive work. Even then, there is the question of *genera conservanda* and I believe that, whatever work or date is adopted, it will be most desirable to adopt a list of *genera conservanda*. There is nothing illogical in this and practically there are great advantages unless one believes in the theory that mere changing of names is a merit in science. That theory I certainly do not accept, but hold that the fewer changes of names the better.

It has been my misfortune never to have found anything perfect. Some of my friends have perfect systems of classifications of books, of herbaria or of plants. In trying to apply perfect methods I always recall a visit in company with Sir Joseph Hooker to an establishment not a thousand miles from here. The person in charge said, "We think we have a perfect museum case which we would like to show you." "Yes," said Sir Joseph, "I am always glad to see what I have never seen. But what do you keep in the case?" A key was produced, but by no amount of coaxing or forcing could the case be opened. "Yes," said Hooker, "I presume that it is perfect, but I prefer cases which open." The same remark would apply to a good many systems. They are perfect until we try to find out what is in them.



## NOTES ON SOME BEAUFORT FISHES—1909<sup>1</sup>

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THE following observations were made while working at the Station of the Bureau of Fisheries at Beaufort, N. C., from May 26 to July 6, 1909, and are recorded in brief in the card catalogue of fishes in the laboratory. While they are but *membrana disjecta*, they are published in the hope that they may not be devoid of value to those interested in fishes.

These fishes, with the exception of the ones specifically noted from other localities, were taken at the Narrows of Newport River. This is a small stream, whose sunken lower valley is an estuary opening into and forming a part of Beaufort Harbor. The Narrows, distant some seven miles in a northeasterly direction from the laboratory, are at the head of the estuary. Here are a number of "rocks" or reefs built by oysters out on the mudflats. Their names, as one comes to them in going up stream, are Lawton's, Cross and Rockfish Rocks, and a fourth one not named. These reefs, extending out at nearly right angles from the shore, give the channel a very tortuous course. Above them the mudflats spread out so shallow that the river is not navigable save for small boats and by them only at high water. The waters of the ebb tides, collected off these mudflats and largely confined by the reefs to this narrow channel, surge around these points and have scoured out deep holes. Here are to be found large numbers of various fishes, which go up the river to feed on the mudflats and at low tide drop down into these holes. There is no seining ground at Beaufort, known to the writer, where so many different species of fishes may be taken at a single haul of the seine as at the "Rocks" at the Narrows of Newport River.

It is interesting to note that the water at the Narrows

<sup>1</sup> Published by permission of the Commissioner of Fisheries.

is markedly brackish, owing to the large amount of fresh water brought down by the river. This, coming from the cypress swamps a few miles away, has a decidedly yellowish tinge, the so-called "juniper water." In density this water ranges from 1.0072 just after one of the tropical downpours to which Beaufort is subject, to 1.0184 when only the normal amount of fresh water is brought down. Ten observations for last summer, which was a normal one from the standpoint of rainfall, give an average density of 1.0153. The average temperature of the water at the time of these observations was 23.6° C. at a depth of three to five feet.

From May 26 to June 12 we constantly caught large numbers of small menhaden, *Brevoortia tyrannus*, averaging about four inches in length. These are probably young of the previous fall spawning. The average size of adult menhaden is about 12 inches. The largest ever taken at Beaufort by the writer was 15 inches long. The record fish for the Atlantic coast is 18 inches in length.

The writer has taken hundreds of gaff topsail catfish at the Narrows, some quite large, but the record was broken on May 27 by the capture of a female, whose length from tip of nose to tip of caudal fin was 24½ inches, and whose girth back of the dorsal was 13 inches. The abdomen was distended balloon fashion by the enormous ovary which occupied almost all of its interior, crowding and displacing the stomach and intestine as it enlarged. This ovisac, after being three days in 10-per-cent. formalin, measured 7½ inches in length and 9¾ inches in girth, and weighed 435 grams.

On May 27, at Cross Rock, we took a large tripletail or flasher, *Lobotes surinamensis*. Its length all over was 25 inches, its depth (body only) 10½ inches. Unfortunately there was at hand no means for weighing it. Other large fish of this kind recorded in the card catalogue at the laboratory are, one 21 inches long and 8½ inches deep, another 18½ inches long, a third 18 inches long. All were taken by the writer; the last in the pound net two miles up Newport River, the others at the Nar-

rows. Smith in his "Fishes of North Carolina"<sup>2</sup> notes three large specimens from Beaufort: one of 25 inches, another of 23 inches, and a third (length not given) which weighed 11 pounds.

On May 31, a 14½ inch specimen of the cutlass-fish, *Trichiurus lepturus*, was taken at Rockfish Rock. On June 5, another specimen, 15¾ inches long over all, was taken at the same place. These young fish showed clearly the oral breathing valve, which is perhaps more plainly seen in the mouth of the adult than in any other fish known to the writer. One of these fish had had a bite taken out of its dorsal fin and region just over the hinder edge of the pectorals. This has failed to regenerate and the wound had healed, leaving a crescent cut out of the fin.

Not only are these the smallest specimens of this fish ever taken at Beaufort, but they are further interesting because of the fact that both so amply justify the name "hair-tail," since in each the tail is prolonged backward in a long delicate whiplash-like organ, much slenderer in proportion to the size of the body than is the case in the adult. They were probably two, quite certainly not more than three years old. The writer has obtained eggs by "stripping" this fish in August, and sperms from males caught in July. Nothing, I believe, is known of the embryology of the fish, but Lutken,<sup>3</sup> in 1880, figured and described the young of two allied forms, *Gempylus serpens* and *Neolotus tripes*.

On the same date and at the same place as the preceding, there were taken four specimens of the cow-nosed ray, *Rhinoptera bonasus*. These were all about of one size, averaging 24 inches wide. A female after being clubbed on the head until insensible, gave birth, while being cut open, to two young measuring 8½ inches long and 13½ inches wide. This premature delivery of the young, brought about by muscular contraction due to re-

<sup>2</sup> Smith, H. M., "The Fishes of North Carolina," N. C. Geological and Economic Survey, Raleigh, 1907.

<sup>3</sup> Lutken, Chr., "Spoila Atlantica," *Danske Videnskabernes Selskabs Skrifter*, 5te Raekke, Natur. og. Math. Afdeling, Table III., figures 3-8.

flex action, is by no means unusual, the writer having noticed it on several occasions in both butterfly and sting rays. Not infrequently, however, the young are born while the mother is being killed, the pain causing spasmodic contractions of the muscles of the uterus.

In this connection it is interesting to read in Schomburgk<sup>4</sup> as follows:

I have frequently observed that the rays, no doubt in consequence of the anguish when secured and transfixed by the poles (harpoons), brought forth their young ones.

A similar occurrence was once noticed by Dr. S. West-ray Battle, of Asheville, N. C., who related it to the writer. The young slip very readily out through the genital orifice, and on several occasions (July 29, 1902, for the first time), I have delivered the mother of her young by manipulating her abdomen in the manner familiar to spawn takers. The young are rolled up in tubes, one pectoral fin forming the inner lining of the tube, the other the outer; *i. e.*, the fish is rolled up like a sheet of paper beginning at the edge of one pectoral. It is interesting to note that the teeth of these young rays were hard and fit for service, and the spines able to produce a wound.

On May 29 I found on Fort Macon Beach, about one half mile south of the concrete breakwater, a dead and half dried specimen of *Raja eglanteria*, called "brier ray" because of the curved prickles with which its dorsal region and especially its tail are covered. This is the "clear-nose" of the fishermen, and the dried specimen in question fully justified the appellation, since the membrane joining the rostral cartilages to the pectoral fins was translucent almost to the point of transparency. The fontanelles of the skull, especially the anterior one, were clearly marked out. The total length of this specimen from tip of nose to broken-off end of the tail was 19 inches: the pectorals were rolled up and so hard that it was impracticable to ascertain the width of the fish.

This is the only specimen of the ray which the writer

<sup>4</sup>Schomburgk, R. H., "Fishes of Guiana," Part II., in Jardine's Naturalist's Library, 1843.

has seen, and the only one noted in the card catalogue at the laboratory. Of this species, Smith<sup>5</sup> wrote in 1907:

It has not previously been recorded from North Carolina, although it doubtless occurs along the entire coast of the state. At Cape Lookout, on April 22, 1904, the author observed numerous specimens on the beach and was informed that many are caught in the deep-water gill nets set in that region.

On June 5 a three-foot male *Scoliodon terranova*, the ordinary sharp-nosed shark found everywhere in the harbor, was captured at Lawton's Rock. Noticing that he had the tail of an ordinary eel, *Anguilla chrysypa*, sticking out of his mouth, I opened him and found in his stomach the half-digested remains of two other eels, smaller than the first. This observation leads one to conjecture whether eels constitute a steady article of diet for sharp-nosed sharks.

Lest it should seem strange for the shark to be taken and to die in the boat with this half-eaten fragment of food in his mouth, it may be of interest to add that I have seen in the hold of a menhaden schooner, sharks of the same kind literally full of menhaden, stomach, gullet and mouth; and with menhaden impaled on the teeth half in and half out of the closed mouth. The menhaden fishermen report that this is a very common experience with them.

The spotted sting ray, *Aetobatus narinari*, was first described from Brazil by George Marcgrave in his "Historiæ Rerum Naturalium Braziliae," published in "Historia Naturalis Braziliae" by William Piso and George Marcgrave at Lugduni Batavorum et Amstelodami, in 1648. In his description of this ray, Marcgrave gives a figure which is perfectly recognizable and indeed is admirable, considering the time when it was drawn.

Yarrow<sup>6</sup> reports that in his day this ray was very common at Beaufort. But so rare is this fish there at the present time that, in seven summers' fishing, I had never seen one until a female was taken at Rockfish Rock on

<sup>5</sup> Smith, H. M., *op. cit.*

<sup>6</sup> Yarrow, "Notes on the Natural History of Fort Macon, North Carolina, and Vicinity," No. 3—Fishes, *Proc. Acad. Nat. Sci. Philadelphia*, Vol. XXIX, 1887.

June 12, and then I was quite sure that I had found a new species until the director of the laboratory, Mr. Henry D. Aller, positively identified it under the above name. Its width across the utmost reach of the pectorals was 26 inches; length, end of snout to tip of ventrals, 18 inches; length of tail, root to tip,  $40\frac{1}{2}$  inches; tail with two spines, length over all  $56\frac{1}{2}$  inches (tail inserted between bases of ventrals); width between eyes 4 inches.

The most interesting thing about this ray apart from its color is the very unusual structure of its jaws. Whereas, in ordinary rays with pavement teeth, the upper and lower jaws are practically duplicates, in this ray while the upper jaw is of the ordinary shape, the lower is drawn out into a tongue-shaped organ protruding beyond the lips. These would seem to be especially fitted for cracking clams, which dissection proved to constitute its chief food. It should be remarked in passing that the jaws of *Aëtobatus* are noticeably smaller than the jaws of an ordinary ray of the same size.

On July 3 Mr. Russell J. Coles, of Danville, Va., caught in the bight of Cape Lookout and very kindly brought to the laboratory another ray of this species. This was also a female whose measurements were as follows: Width,  $23\frac{1}{2}$  inches; length, body only, 16 inches tail,  $35\frac{1}{2}$  inches long and provided with two spines; width between eyes,  $3\frac{1}{2}$  inches. Mr. Coles, notwithstanding his long experience as a fisherman for sharks and rays at Beaufort and Cape Lookout, thought that he too had caught a new species.

As a further evidence of the scarcity of this ray in the Beaufort region, it may be noticed that the card catalogue of the laboratory has records of but two other specimens. In 1901 some fishermen took on the outer or channel side of Shark Shoal, in the deepest part of Beaufort Harbor an *Aëtobatus narinari* whose dimensions were as follows: Width, 4 feet; length, nose to ventrals, 2 feet and 2 inches; tail, 4 feet and 8 inches long; total length about 6 feet and 6 inches (allow for insertion of tail between ventrals). On August 3, 1909, Mr. C. F. Silvester, of Prince-



ton, N. J., took a specimen inside the hook of Cape Look-out. Of this, however, no measurements were made.

Considerable numbers of sting and butterfly rays, *Dasyatis say* and *Pteroplatea maclura*, were taken at the Narrows. The females of these were commonly found to have gravid uteri. At the beginning of my season's seining, the eggs had on them young in very early stages not longer than 10-12 millimeters, the "selachian stages" of Alcock. Two eggs were found having what seemed to be membranous egg shells like those found on teleost ova, and one had the ends twisted into chalaza-like structures similar to those in a hen's egg. Alcock seems to have found similar structures in *Carcharias melanopterus*, a viviparous shark of the Indian Ocean. He writes:

Each young one lay, head forwards, in its own separate compartment of the uterus, in which, further, it was completely enveloped in a very delicate membrane of its own. This delicate envelope is evidently the pseudamnion of Parker, which, according to that author, corresponds with the horny egg-shell of viviparous Elasmobranchs.

Those uteri, which contained embryos having the yolk sac gone or reduced to a mere string, had their whole inner surface crowded with villi, shaggy with them. Both the villi and the deeper layers of the uteri were tremendously vascularized, and the whole uterus in any specimen was greatly distended by this unwonted blood supply. Uteri containing young as above described were filled with and had the young bathed in a milky fluid evidently secreted by the villi (in the absence of a placenta) as nourishment for the embryos until the time of hatching. According to Alcock this "milk" is taken through the open spiracles of the Indian *Pteroplatea micrura*, a closely allied form, into the mouth, stomach and intestine. Since the Beaufort species likewise have the spiracles wide open, it is fair to suppose that they too feed in the same way. This fluid, somewhat creamy in appearance, was greasy to the touch, and readily coagulated when put into preserving fluids like formalin. I regret that no

<sup>1</sup> Alcock, A. W., "Zoological Gleanings from the Royal Indian Marine Survey Ship Investigator," "Scientific Memoirs by Medical Officers of the Army India," Part 12, 1901.

particular microscopical examination of this "milk" was made. These observations fully confirm those of Alcock<sup>8</sup> on Indian Ocean rays in the "Investigator" expeditions. They are necessarily incomplete, but it is the purpose of the writer to make a careful study of the phenomenon of viviparity in sharks and rays.

*Lepisosteus osseus*, the "shell gar" of the fishermen (so called to distinguish it from the green gar, *Tylosurus marinus*, formerly *Belone longirostris*), often comes down from the swamps and upper reaches of Newport River into the brackish water at and even below the Narrows.

In 1908 my fishermen caught several at Cross Rock. Of these, three were thrown on the grated bottom of the gasoline launch, and, with no further attention than having an occasional drenching with a bucket of salt water and being covered from the sun's rays by the corner of an old sail, were brought to the laboratory. Here they were put in a large tank of fresh water, which was aerated only semi-occasionally by a jet from a pet-cock. As a result of this severe experience, one of the fish died. However, the other two survived, though their pectoral and caudal fins were badly split and were congested with blood as a result of their threshing around on the floor of the boat during the journey of more than an hour from the fishing ground to the laboratory. Further, in a short time the fins of the living fish became much worn by contact with the scaling paint of the tank.

These two gars came safely through the winter of 1908-09, thanks to the care given them by the laboratory men under the direction of the superintendent of the station, Mr. Henry D. Aller. On my return to Beaufort on May 26, 1909, I found that the fish had completely regenerated their fins and that these were in as good physical condition as the day, ten months before, when the gars were taken out of the bunt of the seine. However, the larger of the two had the body curved in a curious very flat S, which led me to think that it had suffered some injury resulting in deformity. On June 4 this crook had disappeared and the fish seemed to be perfectly

<sup>8</sup> Alcock, A. W., *op. cit.*; "A Naturalist on Indian Seas," 1902.

normal. Later in the day my attention was called by one of the laboratory men to some spawn floating near the bottom of the tank. This was examined and found to have a greenish color, and undoubtedly was the eggs of one of the gars, presumably the larger. The crook in the body of this fish was probably the result of an effort on her part to rid herself of the eggs without the aid of the sexual excitement engendered by the presence and activity of a male as noted by Dean.<sup>9</sup> The eggs had gone bad—strong evidence that the smaller fish was also a female. When I left Beaufort, at the end of the first week in July, the fish were in prime condition.

Although the water in the tank (size 3 feet by 8) in which the fish were confined, was not more than six inches deep and was aerated only semi-occasionally, the fish did not seem to suffer, thanks to their vascularized air bladders. I watched these gars at irregular but fairly frequent intervals during my six weeks' stay, and noticed that the larger fish seldom came to the surface for air, but that one was quite sure in a few minutes' watching to see the smaller do so. Certain preliminary symptoms always preceded this action: the fish would swim around slowly but uneasily, would shake its head from side to side, would raise its snout nearly to the surface of the water and then sink down; finally with almost imperceptible rapidity the snout would be thrust out of the water and the jaws would open and shut with a convulsive snap; then, as the fish slowly sank to the bottom of the tank, the gill covers would expand and one or two large bubbles of air would escape from under each operculum.

The fish were fed simply by putting into the tank live minnows, chiefly *Fundulus*. These seemed much frightened and would huddle in a corner of the tank as far from the gars as possible. In feeding, a gar would slowly swim behind a *Fundulus*, make a sudden dash, and seize it crosswise in its long toothed jaw. Then elevating its head, possibly out of water, with a quick jerk the gar would catch the fish head first and speedily swallow it.

<sup>9</sup>Dean, Bashford, "The Early Development of Garpike and Sturgeon," *Journal of Morphology*, Vol. 11, 1895-96.

## ON THE EFFECT OF EXTERNAL CONDITIONS ON THE REPRODUCTION OF DAPHNIA<sup>1</sup>

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SINCE in the great majority of organisms only the germ cells are capable of reproducing the entire individual, the question as to what differences exist between the germ and body cells, and how they arise, is of general interest. Therefore any change of conditions which affects the germ cells or their relation to the body cells deserves special study. In *Daphnia* external conditions not only affect the relation of the germ cells to the body cells, but they affect the egg cells in such a manner as to determine whether they do or do not need fertilization. The purpose of the present paper is not merely to add the results of my experiments to those of other investigators, but to tentatively arrange the available data under a general working hypothesis in the hope that some more direct method of investigating the relation of the germ and body cells be devised.

Last spring (March 10) I began experiments on the effects of environment on *Daphnia pulex*, De Geer, without knowing that Woltereck was working on the same line. The material came from a small pool investigated by Dr. W. C. Curtis and containing a single strain of this and no other species of *Daphnia*. For the first few weeks some ice remained on the pool and the temperature did not much exceed 4° C.; after this it rose steadily to about 20° by the end of May. Specimens from the pool were examined at intervals as a control on the experiments.<sup>2</sup>

<sup>1</sup> From the Zoological Laboratory of the University of Missouri and the Histological Laboratory of Cornell University Medical College, New York City.

<sup>2</sup> When the daphnids were crowded in dishes of the same pool water they soon began to die, owing to the accumulation of their excretions. When

## EFFECT OF ENVIRONMENT ON DIFFERENTIAL GROWTH

By differential growth I mean the unequal growth of different parts, viz., the germ and body cells. Only parthenogenetic females were used, and each was kept separately in the same quantity of water. All measurements were made at sexual maturity, *i. e.*, when the first eggs appeared in the brood pouch. Warren<sup>3</sup> found that under uniform conditions there was a slight variability, but Woltereck showed that these fluctuating variations were very small, though he did find mutations as rare occurrences.

*Nutrition.*—I had in the laboratory a pure culture of a unicellular green alga which the daphnids ate readily. This alga did not remain entirely suspended in the water, but as the daphnids fed on the bottom as well as while swimming, and stirred up the algæ, it can not be said that most of the food was out of their reach.

Those with a superabundance of food were larger at sexual maturity and had a shorter spine than those with insufficient food, and conversely. The smaller size and longer spine of the starved daphnids are characteristic of immature stages.

*Temperature.*—Three sets of experiments were transferred suddenly to artesian tap water many died, though with a gradual change all lived. The composition of the tap water was as follows: Ca,  $.148 \times 10^{-8}$  molecular; Mg,  $.1 \times 10^{-8}$  molecular;  $\text{CO}_2$ ,  $.045 \times 10^{-8}$  molecular;  $\text{SO}_4$ ,  $.146 \times 10^{-8}$  molecular; Cl,  $.055 \times 10^{-8}$  molecular. Besides these were very small quantities of silica, clay, iron, ammonia and nitrates, and traces of lithium and potassium. Beside the carbonates the water when drawn from the tap was super-saturated with carbon dioxide. In order to find the cause of death from change of water I added various amounts of molecular solutions of certain salts. The toxicity of cations increased as follows:  $\text{Na} < 1/2 \text{Ca} < \text{K}$ , and of anions:  $\text{Cl} < 1/2 \text{SO}_4 < \text{HCO}_3 < 1/2 \text{HPO}_4$ . But as K became toxic only on 1/100 and  $\text{HPO}_4$  on 1/500 molecular concentration there must be other toxic substances than salts in the water. The carbon dioxide in the water killed crayfish and was probably the most toxic constituent to the daphnids.

<sup>3</sup>"An Observation on Inheritance in Parthenogenesis," *Proc. Roy. Soc. London*, 1899, LXV, and "On the Reactions of *Daphnia magna* to certain changes in Environment," *Quart. Jour. Micr. Sc.*, 1900.

started: at 4-10°, 19-20° and 29-31° C. The results were as follows:

	Average Body Length.	Ratio of Average Spine Length to Body Length.	Time from Hatching to Sexual Maturity.
4-10°	22	0.24	35 days
19-21°	20	0.25	14 "
29-31°	17.7	0.27	6 "

1 unit =  $\frac{2}{3}$  millimeters.

All were given a surplus of food daily. It may be observed that a higher temperature has the same effect as insufficient food.

*Salts.*—Since salts have such a marked effect on the development of marine and some fresh-water animals, I placed daphnids in the strongest solutions of various salts that they would live in (without acclimatization). The effects in two months (four generations) were unnoticeable.

*Light.*—Cultures were kept in the dark and in diffuse and direct sunlight, but no effect was observed.

A number of observers have recorded season-polymorphism in daphnids. Wesenberg-Lund<sup>4</sup> pointed out that when the specific gravity and consequent buoyancy of the water decreased—by heat in summer—the body of the daphnids became smaller or were provided with outgrowths, so as to offer a greater resistance to sinking. Wolfgang Ostwald<sup>5</sup> produced, all at the same time, the forms that occurred in nature at different seasons, by varying the temperature. He emphasized the fact that rise in temperature lowered the internal viscosity of the water. He found that in the warm cultures the daphnids often became productive at an undeveloped stage and, as is true generally, reproduction retarded body growth.

<sup>4</sup>“Ueber das Abhängigkeitsverhältnis zwischen den Bau der Planktonorganismen und den specifischen Gewicht des Süßwassers,” *Biol. Centrb.*, 1900, XX, pp. 606-619, 644-656, and “Studier over de Danske søers Plankton,” Copenhagen, 1904.

<sup>5</sup>“Experimentelle Untersuchungen über den Saisonpolymorphismus bei Daphniden,” *Archiv f. Entwicklungsmech. d. Organismen*, 1904, XVII, p. 415.



Woltereck<sup>6</sup> maintains that Ostwald's results were due to the fact that at a higher temperature the daphnids need more food. Woltereck caused decrease in body length both by starving and by increasing the temperature, but the latter was not effective with an optimum supply of food. He found that more food than the optimum produced effects similar to starving. Raising the internal viscosity of the water by adding quince gum produced no effect. He showed that though feeding influenced the differential growth, there was a cyclical tendency for this to vary, viz., season-polymorphism. However, the effects of prolonged abundant feeding were inherited to some degree.

One might interpret these results in different ways. It is known that the temperature coefficients for various chemical reactions are slightly different. Possibly the mean of the temperature coefficients for the processes in the development of the reproductive organs is higher than the same for the body wall, and at a higher temperature the germ cells would develop faster. However, under adverse conditions the "affinity" of the reproductive organs for nutriment is greater than that of the rest of the body, so with deficient food the body wall is retarded more than the germ cells in development. The higher temperature may be considered an adverse condition since the mortality is greater. In this way starving has the same effect as a higher temperature.

Langerhans<sup>7</sup> found that accumulation of excretions caused shortening of the spine in daphnids. I do not know what relation this bears to the above results.

<sup>6</sup>"Ueber natürliche und künstliche Varietätenbildung bei Daphniden," *Verh. Deutsch. Zool. Gesell.*, 1908, p. 234; and "Weitere experimentelle Untersuchungen über Artveränderung, speziell über das Wesen quantitativer Artunterschiede bei Daphniden," *ibid.*, 1909, p. 110.

<sup>7</sup>"Ueber experimentelle Untersuchungen zu Fragen der Fortpflanzung, Variation und Vererbung bei Daphniden," *Verh. Deutsch. Zool. Gesell.*, 1909, p. 281.

## EFFECT OF ENVIRONMENT ON THE LIFE CYCLE

In most species of daphnids, generations of parthenogenetic females alternate with generations of males and females which produce eggs that must be fertilized, and either frozen, dried or kept a long time before they will develop (resting or "winter" eggs). In different species the number of successive parthenogenetic generations varies. In some all are, and in some none are, parthenogenetic.

I found that heat hastened the appearance of sexual forms, as did starving or the accumulation of excretory products. All of these factors might be combined in the drying up of a pond, as heat would aid in drying, and drying would concentrate the daphnids and their excretions, and concentration of the daphnids would cause them to eat up the algæ faster than they could multiply. However, by keeping the culture cold, fresh or well-fed, or all combined, I could delay but not prevent the appearance of sexual forms.

Kurz<sup>8</sup> said the drying up of the water caused the appearance of sexual forms, and Schmankewitz<sup>9</sup> suggested that it was the increase in salts. Weismann<sup>10</sup> tested both of these hypotheses and concluded that they were wrong. He also tried the effect of food and temperature, with varying results. He concluded that the life cycle was fixed for each species and variety. Issakowitz<sup>11</sup> concluded that cold favored the appearance of sexual forms and warmth favored the parthenogenetic. Also, hunger favored the appearance of sexual forms and abundant food the parthenogenetic. It may be that cold retarded multiplication of the food plant or the

<sup>8</sup>"Dodekas neuer Cladoceren nebst einer kurzen übersicht der Cladoceren-fauna Böhmens," *Sitz. Ber. math. naturw. Wien*, 1875.

<sup>9</sup>"Zur Kenntniss des Einflusses der ausseren Lebensbedingungen auf die Organisation der Tiere," *Zeit. wiss. Zool.*, 1877, XXIX.

<sup>10</sup>"Beiträge zur Naturgeschichte der Daphnoiden, VII," *Zeits. wiss. Zool.*, XXXIII, p. 111.

<sup>11</sup>"Geschlechtbestimmende Ursachen bei den Daphniden," *Biol. Centralb.*, 1909, XXV, pp. 529-536.

movements of the daphnids so that they did not keep the algæ stirred up in the water sufficiently to get at them. The parthenogenetic egg arises from four cells, but a large number of cells enter into the composition of the fertilizable egg. If the latter egg is not fertilized it is absorbed and, as Issakowitsch noted, furnishes food for the development of parthenogenetic eggs.

Woltereck<sup>12</sup> found that starving hastened the appearance of sexual reproduction, but a concentration of food above the optimum produced results similar to starving. He found, as Weismann maintained, a cyclical tendency toward the alternation of sexual and parthenogenetic generations which, contrary to Weismann, was temporarily influenced by nutrition, and the effects of constant nutrition over a long period was inherited to some extent.

Langerhans<sup>13</sup> found that the accumulation of excretions caused the decrease in numbers of parthenogenetic females in the autumn and thinks that the appearance of sexual forms is due to the same cause.

The life cycle of a daphnid is, therefore, an hereditary tendency, but can be influenced by nutrition and probably by temperature and the accumulation of excretions. Nutrition is the most important factor, and former experiments on the effect of temperature and the drying up of the water were complicated by secondary effects on concentration of food and excretory products.

*Discussion of Results.*—Two views might be held as to the origin of the differences between the germ and body cells: the differences might be the result of difference in position in the embryo, or of unequal mitoses. In the parasitic copepods I found the primary germ cell to arise by an unequal mitosis at the fifth cleavage of the egg. The germ cell when first formed is one thirty-second of the total number of cells, but owing to the more

<sup>12</sup> *Loc. cit.*

<sup>13</sup> *Loc. cit.*

rapid division of the body cells this ratio decreases. In fact the chief difference between the yolk-free body cells and the (yolk-free) germ cells is the slow rate of division of the latter. Finally, the *eggs* will not divide at all unless specially stimulated, by fertilization. The question now arises: what causes the cell to divide. Sacks found that plant cells divide when they have reached a certain size. This rule has been extended to animals, and the final size of the cell found to be determined by the ratio of nucleus to cytoplasm. This rule may apply to the germ cells, since it appears that after the egg cell, primary oocyte, reaches a certain size any additional food absorbed does not cause growth of the protoplasm, but is precipitated as yolk.

If the egg is properly stimulated, rapid growth of protoplasm and cell divisions follow. From the study of artificial parthenogenesis it appears probable that stimuli which lead to development of the egg increases the permeability of its plasma membrane. If this be true we may say that the germ cells are distinguished by the fact that their plasma membranes are poorly permeable and retard those reactions between the cell contents and environment which lead to growth and cell division. In other words, the optimum intensity of stimulation toward growth and division is higher for the germ cells than for the body cells.

This difference is probably due to a difference in the colloids of the cell, which in animals could be explained as the result of an unequal mitosis. This explanation may be modified so as to apply to plants. Klebs has shown that those conditions which are adverse to vegetative growth of plants (too strong stimuli?) call forth flowers. Perhaps there are slight differences in the sensitiveness of plant cells to stimuli, and as the stimuli increase, those initially least sensitive cells acquire further immunity to the stimulus, whereas those initially more sensitive cells are overstimulated and weakened. Thus the difference between germ and body cells is grad-

ually acquired. The Malpighian layer of the skin may be stimulated to proliferate more rapidly, but if the stimuli are too strong the growth will be retarded instead of increased. On gradually increasing the stimulus immunity to it may be acquired.

To apply this hypothesis to the daphnids: conditions which are adverse to the growth of the body cells, such as extremes of temperature (viz., high temperatures) or of concentration of excretory products, or disordered nutrition, either fail to retard the development of the germ cells or stimulate their development, so that in either case the daphnid becomes sexually mature at a less developed stage. Under the less extreme conditions the eggs develop on receiving the slight stimulus incident to their transfer to the brood pouch, but under the more extreme conditions those eggs which develop at all must be stimulated by fertilization before they develop. These two types of eggs may perhaps develop from two kinds of cells, or the sexual egg may arise from the same kind of cell producing the parthenogenetic egg by acquiring an immunity to slight stimuli. Whereas more than one cell goes to make up a single egg; only one nucleus is retained, and it may be said that one cell is the egg cell and the remainder furnish its food.

## ARE FLUCTUATIONS INHERITED?<sup>1</sup>

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THE object of this paper is to present certain facts in regard to the inheritance of fluctuations which have been obtained from a study of the common garden pea (*Pisum sativum*). While an experiment was being conducted to study the effect of fertility upon the fluctuating variability of certain characters, data were also obtained to show to what extent these fluctuating characters are inherited. The peas used were a mixed population and not a pure line, and had not been selected for any particular character when the experiment was started.

The belief has long been held that the improvement of animals or plants could be obtained by selecting from the best individuals, or those possessing to the greatest degree the quality desired.

The fact is that at present most of our agricultural breeding and improvement is based upon this belief and this is the method which is most generally followed in practical breeding. This view held sway from the time of the earliest breeders until the appearance of the Mutation Theory by DeVries. It was first stated scientifically by Darwin in his "Origin of Species," for Darwin, after a study of the evidence accumulated, was convinced that the improved breeds had been obtained in this manner. He says:

We can not suppose that all the breeds were suddenly produced as perfect and as useful as we now see them; indeed, in many cases we know that this has not been their history. The key is man's power of accumulative selection: nature gives successive variations; man adds them up in certain directions useful to him. In this sense he may be said to have made for himself useful breeds.<sup>2</sup>

<sup>1</sup>Contribution VI, Laboratory Experimental Plant-breeding, Cornell University. The writer expects to follow with a series of articles on the same subject.

<sup>2</sup>"Origin of Species," Chapter 1, p. 35, Murray Edition.



He further states:

If selection consisted merely in separating some very distinct variety, and breeding from it, the principle would be so obvious as hardly to be worth notice; but its importance consists in the great effect produced by the accumulation in one direction, during successive generations, of differences absolutely inappreciable by an uneducated eye—differences which I for one have vainly attempted to appreciate.\*

DeVries after much careful experimentation and study of results of agricultural breeding (mainly the results at Svalöf and those of the German breeders) showed that much of the improvement which has occurred was not due to cumulative selection but must be explained in some other manner.

In recent years much careful scientific work has been done along this line to test the action of selection within a pure line and in a mixed population.

The Svalöf Experiment Station has done much in practical plant-breeding to show that, with reference to the cereals especially, the effect of selection went no further than to isolate the pure lines, and when this was accomplished no further gain was made by selection.

Johannsen,<sup>4</sup> working with beans and Jennings<sup>5</sup> with *Paramecium*, have arrived at the same general conclusion which has been summed up by Pearl<sup>6</sup> as follows:

From a mixed "general" population it is possible by a single selection to isolate pure strains ("pure lines," "homozygote strains," "pure races") which will breed true and not revert to the mean of the general population from which they were isolated, regardless of whether further selection is practised or not. It is impossible to demonstrate any cumulative effect of continued selection within the pure strain. Continued breeding from the extreme individuals of such a pure strain ("fluctuating" variants) does not change the mean of that strain. From these considerations it follows that it will be difficult or impossible to make any definite and permanent change in the mean of a general population simply and solely by continued selection of extreme indi-

\* *Loc. cit.*, p. 36.

<sup>4</sup> "Über Erblichkeit in Populationen und in reinen Linien," Jena, 1903.

<sup>5</sup> "Heredity, Variation and Evolution in Protozoa, II," *Heredity and Variation of Size and Form in Paramecium with Studies of Growth, Environmental Action and Selection*, *Proc. Amer. Phil. Soc.*, Vol. XVI, pp. 393-546, 1908.

<sup>6</sup> "Is there a Cumulative Effect of Selection?" *Abstammungs- und Vererbungslehre*, 2, 1909, H. 4.

viduals, because in the vast majority of cases such individuals will be extreme fluctuating variants rather than mutants.

Jennings<sup>7</sup> states in his paper that "Systematic and continued selection is without effect in a pure race, and in a mixture of races its effect consists in isolating the existing races, not in producing anything new." And concludes<sup>5</sup> that "Until some one can show that selection is effective within pure lines it is only a statement of fact to say that all the experimental evidence we have is against this."

Recently Pearl<sup>6</sup> has brought forth a very noteworthy contribution in this line. His evidence is based upon the work which is being done at the Maine Agricultural Experiment Station to determine the effect of selection on fecundity and the inheritance of fecundity in poultry. His conclusions may be summed up as follows:

Selection for high egg production carried on for nine consecutive years did not lead to any increase in the average production of the flocks.

The present data give no evidence that there is a sensible correlation between mother and daughter in respect to egg production, or that egg-producing ability (fecundity) is sensibly inherited.

In this experiment the daughters of "200-egg" hens did not exhibit, when kept under the same environmental conditions, such high average egg production as did pullets of the same age which were the daughters of birds whose production was less than 200 eggs per year.

Professor Waugh,<sup>8</sup> of the Massachusetts Agricultural Experiment Station, has been making some studies of the variation of peas and the inheritance of the different fluctuating characters. He has found that the coefficients of heredity for the different characters are very low and are not very significant in the case of any one character. The two characters, length of vine and number of pods per vine, show a coefficient of heredity of .170 and .158, respectively. At the same time he found that there were certain lines that did reproduce their characters to an appreciable extent, which is along the

<sup>7</sup>"Heredity and Variation in Simplest Organisms," *AMER. NAT.*, June, 1909.

<sup>8</sup>Twenty-first Ann. Rept. Mass. Agr. Exp. Sta., Part II.

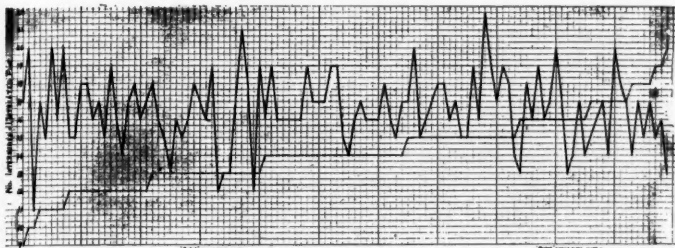


FIG. 1. Diagram showing the relation between parents and offspring in regard to number of internodes on the untreated plot. The broken line represents the parents and the solid line the offspring.

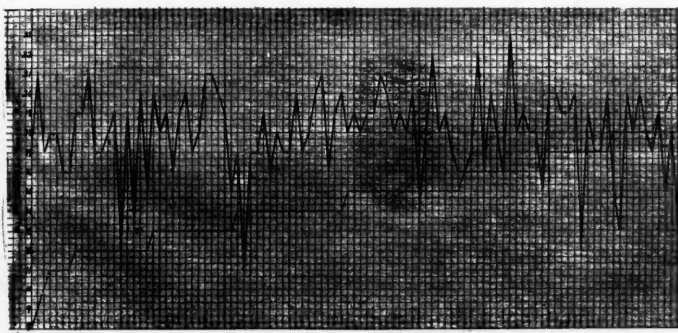


FIG. 2. Diagram showing the relation between parents and offspring in regard to number of internodes on the treated plot. The broken line represents the parents and the solid line the offspring.

line of other studies dealing with pure lines which have been reported.

The evidence which the writer here presents is not based upon large enough numbers to be conclusive in itself, but it is certainly suggestive and adds to the facts already brought together. The number of individuals used is as large as is used many times in the beginning of a practical experiment to develop a strain by selection.

*Plan of the Experiment.*—The writer studied the variability of peas when grown on plots of different fertility, and in carrying this study on a second generation the planting was done in such a way that the parentage of each plant was known. That is, each plant of the

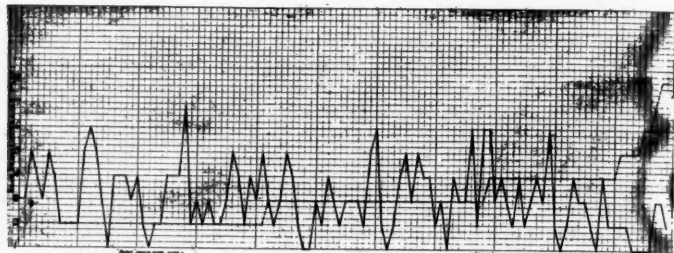


FIG. 3. Diagram showing the relation between parents and offspring in regard to number of pods on the untreated plot. The broken line represents the parents and the solid line the offspring.

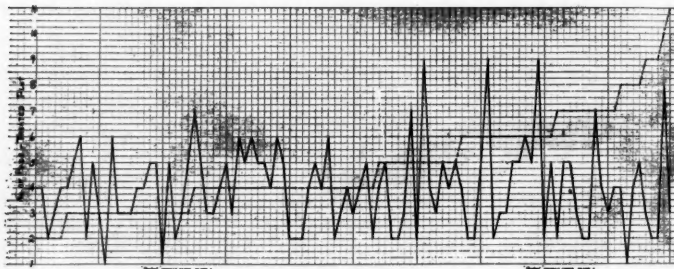


FIG. 4. Diagram showing the relation between parents and offspring in regard to number of pods on the treated plot. The broken line represents the parents and the solid line the offspring.

second generation plants was planted from certain parent plants, and the records were kept in such a way that the characters of the offspring could be compared with those of the parents. As stated above, the number of individuals was of necessity small, yet since the study was made under two different soil environments it seems quite representative of what one is to expect in ordinary selection work.

The difference between this study and regular selection work is that no selection of parents was made, while in ordinary selection work the best individuals are saved for planting.

These data are presented to answer the questions—"What basis have we to expect a cumulative effect of selection? Do we find the resulting offspring following the law of regression?"

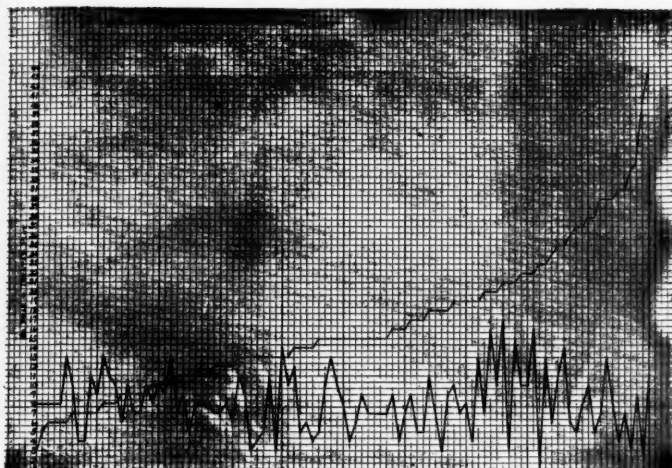


FIG. 5. Diagram showing the relation between parents and offspring in regard to number of peas on the treated plot. The broken line represents the parents and the solid line the offspring.

Regression tables were made for several characters obtained from the plants studied. Such characters as, height of plant, number of internodes, number of pods, number of peas and yield of seed were observed. The results of these calculations are shown in Table I.

TABLE I

Character	Coefficients of Heredity	
	Ordinary Soil Plot	Fertilized Plot
Number of internodes .....	.027 $\pm$ .064	-.050 $\pm$ .067
Number pods per plant .....	-.235 $\pm$ .061	.012 $\pm$ .067
Number peas per plant .....	-.152 $\pm$ .063	-.045 $\pm$ .067
Height of plant .....	-.191 $\pm$ .062	.014 $\pm$ .067
Yield of plant (peas in grams) .....	-.100 $\pm$ .064	.001 $\pm$ .067

We see that the coefficients of heredity are very low and some of them are even negative. This shows that as far as this data is concerned these fluctuating characters do not follow the law of regression to any appreciable extent. No emphasis should be placed upon the fact that some of these coefficients are negative, but the point is that they are not positive to any noticeable degree. The charts (Figs. 1-9) show in a graphic manner



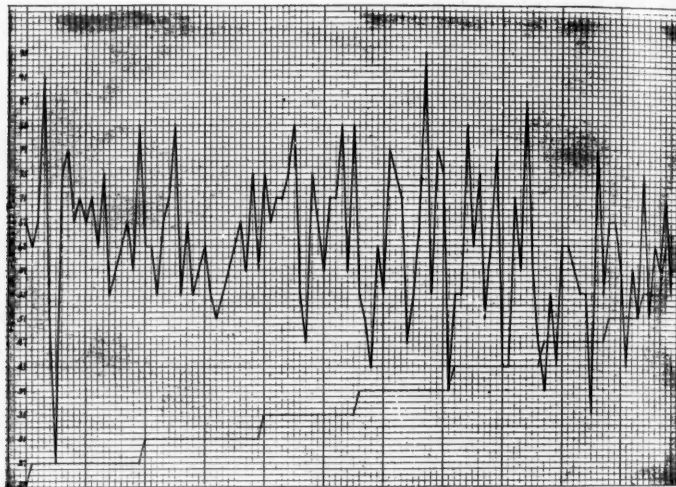


FIG. 6. Diagram showing the relation between parents and offspring in regard to height on the untreated plot. The broken line represents the parents and the solid line the offspring.

the lack of inheritance for these different characters. In each case the dotted line represents the parent plants and the solid line the offspring. These point out some very interesting facts. While we find that in general the coefficients of heredity are practically negligible quantities, yet there are certain parents which reproduce some of their characters. This we are to expect from our knowledge gained by the study of populations. That is, in the study of a mixed population we expect to find certain lines which will reproduce to a marked degree.

The charts show that the line representing the offspring for any certain character seems to rise or fall independently of the value of the parental plant. These diagrams show how in some cases the line for the offspring tends to go down rather than up, but no conclusion must be drawn from this other than that it does not follow the parent line and rise as the parent line does.

Often there arises a question in the minds of those believing in cumulative selection, which may be stated as follows: If we take any population and group it ac-



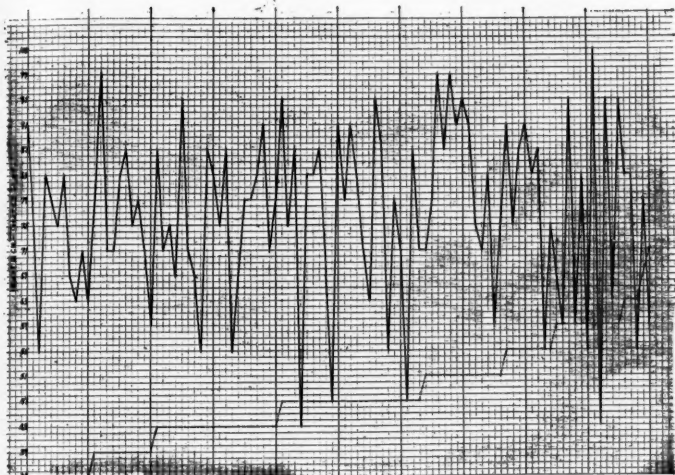


FIG. 7. Diagram showing the relation between parents and offspring in regard to height on the treated plot. The broken line represents the parents and the solid line the offspring.

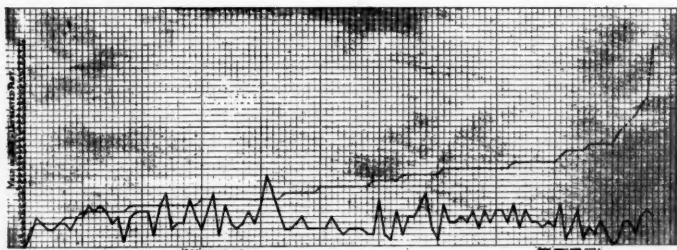


FIG. 8. Diagram showing the relation between parents and offspring in regard to yield on the untreated plot. The broken line represents the parents and the solid line the offspring.

according to any certain character and select from above or below the mean, will not the progeny resulting from the individuals above the mean possess this character to a greater degree than those coming from parents below the mean?

To answer this the writer has arranged the data in the following manner: The individuals used as parents were divided into two parts, the lower half, or the half possessing the character in question in the least degree,

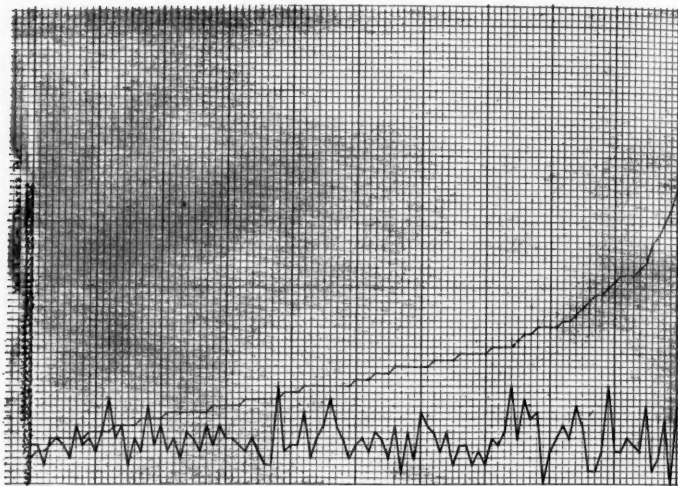


FIG. 9. Diagram showing the relation between parents and offspring in regard to yield on the treated plot. The broken line represents the parents and the solid line the offspring.

and the upper half. The resulting offspring was again averaged for the different characters and the results are shown in Table II.

TABLE II  
AVERAGES FOR THE DIFFERENT CHARACTERS

Character,	First Half.		Second Half.	
	Parent.	Offspring.	Parent.	Offspring.
Height, untreated .....	30.6	65.3	45.0	60.7
"    treated .....	41.2	75.4	53.7	77.7
Internodes, untreated .....	12.6	16.5	15.8	16.5
"    treated .....	12.8	17.9	16.5	18.0
Number pods, untreated .....	2.2	3.2	3.9	3.0
"    "    treated .....	3.4	3.9	6.3	4.0
Number peas, untreated .....	6.4	5.9	12.7	4.8
"    "    treated .....	10.5	8.3	22.2	8.1
Yield, untreated .....	1.493	1.048	3.011	.889
"    treated .....	2.26 <sup>0</sup>	1.476	4.986	1.453

Again, the individuals were divided into three parts as follows: The first third, or those in which the average of any character for the parents was the lowest, the

second or middle third, or those near the mean, and the third third, or those in which the average for the parents was the highest. The average was obtained for the different characters in the offspring produced from these different lots of parents and the results tabulated and shown in Table III.

TABLE III  
AVERAGES FOR THE DIFFERENT CHARACTERS

Character.	First Third.		Second Third.		Third Third.	
	Parent.	Off-spring.	Parent.	Off-spring.	Parent.	Off-spring.
Height, untreated .....	28.7	64.1	36.8	65.4	48.	59.6
"    treated .....	39.2	75.1	46.6	77.1	56.8	77.5
Internodes, untreated .....	12.1	16.2	14.1	16.8	16.4	16.4
"    treated .....	12.1	17.7	14.6	18.3	17.2	17.8
Number pods, untreated...	2.	3.3	2.9	3.1	4.4	2.8
"    "    treated .....	3.1	3.9	4.4	4.1	7.1	4.0
Number peas, untreated...	5.6	5.6	8.8	5.8	14.2	4.7
"    "    treated .....	8.9	8.5	15.	7.3	25.5	8.8
Yield, untreated .....	1.311	1.044	2.100	.972	3.343	.889
"    treated .....	1.941	1.453	3.335	1.453	5.688	1.488

We see that there is no general increase for the value of the different characters as we pass from the lower half to the upper half, or from the lower third to the middle or upper third. In some instances there is an increase, in others a decrease. That is, the individuals resulting from the parents above the mean do not possess the character to any greater degree than those resulting from parents below the mean. We see then that these data answer the question in the negative; or to take a concrete case, the number of peas per plant on the untreated plot show that plants resulting from the upper half of the parents *do not produce* any more peas per plant than those coming from the lower half. The results are as follows:

Average of Parents  
6.4  
12.7

Average of Offspring  
5.9  
4.8

The two tables show very clearly that as far as this data are concerned, there is no difference in the offspring resulting from high or low averaging parents.

As peas are self-fertilized, it will be of interest to note some results obtained with a cross-fertilized crop. The writer has obtained data from selection experiments in corn which are very interesting in this connection. While selecting for yield and earliness data have been obtained which tend to show that corn is not different from peas and that in general fluctuations are not inherited, but that certain individuals reproduce to a high degree. The results obtained with yield are shown in the following table.

TABLE IV

	Average Yield of Parents	Average Yield of Offspring
Plot 1 .....	46.6 lbs. per row	43.0 lbs. per row
Plot 1 .....	52.5 lbs. per row	44.8 lbs. per row
Plot 2 .....	27.2 lbs. per row	81.5 lbs. per row
Plot 2 .....	34.1 lbs. per row	76.2 lbs. per row

While making these studies the effect of the size of seed planted on the offspring was determined. In planting the second generation plants, unfortunately, the seeds were not weighed, but an average seed from each parent was taken and planted. The chances are that by selecting in this manner an average sized seed would be obtained, and since the average weight seed for each parent plant is known, it seems fair to assume that the seed planted approached the average weight of seed. Regression tables were arranged for the two plots in which the height of plant for the offspring was correlated with the average weight of seed for the parent plants. These tables show that as the average weight of seed planted increases, the height or size of the resulting plant also increases. Although the coefficient is not high in either case, yet it is higher on each of the plots taken than that determined for any character. This seems to show that the size of seed, regardless of the plant from which it came, has more influence on the offspring than the parent plant itself. The coefficient is  $.276 \pm .059$  for the untreated plot and  $.139 \pm .066$  for the treated plot.

Waldron<sup>9</sup> has shown that large (heavy) seed in oats

<sup>9</sup>"A Suggestion Regarding Heavy and Light Seed Grain," AMER. NAT., Vol. XLIV, January, 1910.

comes from the shorter culms and suggests that in selecting large seed for planting we are selecting from small plants. This may be true. The writer finds that the average weight of seed in peas decreases with the height of plant, which corresponds to the results obtained by Waldron. On the other hand, when these are planted the larger seed, although coming from smaller (shorter) plants, produced larger plants than smaller seed which came from the large plants.

The foregoing results then indicate that there is not enough evidence in favor of the inheritance of fluctuations caused by environment to form a practical working basis. That is, by selecting out the individual plants which give an exceptionally high yield we would not obtain any higher yielding individuals than from a selection taken at random.

We find then that we have given a definite answer to the question which is the title of this paper.

While such statements do not accord with those who place their faith in cumulative selection, yet it is only a statement of fact as shown by an analysis of such data as are here brought together.

These results accord very closely with those stated by Pearl<sup>6</sup> and we can agree with a statement made by him which is as follows:

Altogether much evidence is accumulating from widely different sources to show that simple selection of superior individuals as breeders can not alone be depended upon to insure definite or continued improvement in a strain. Some improvement may possibly follow this method of breeding at the very start but the limits both in time and amount are very quickly reached.

The rapidly accumulating facts in this respect bring us to face a different view of the value of selection. The testing of individuals to learn their power to reproduce their characters must be done just the same but a different interpretation must be given the results obtained.

Unless further studies produce different results, we can say from the facts at hand that there is no evidence to show that a basis exists for cumulative selection.

## INHERITANCE IN POTATOES

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A STUDY of the behavior of certain plant characters in inheritance formed part of an investigation into the factors connected with the improvement of the common potato as a commercial crop, begun at the Connecticut Agricultural Experiment Station in 1906. This work was really a continuation of investigations made by the writer at the University of Illinois from 1902 to 1905, along broader and somewhat different lines. In 1908<sup>1</sup> the many disheartening difficulties attending hybridization were discussed, but it was shown to be possible to overcome several of the obstacles by proper treatment. The conclusions drawn at that time have not been changed by further experience, but the hindrances caused by external conditions not under control have been so great that the work has been discontinued. For example, in 1908 a prolonged drought at the time the fruits were forming, caused one hundred and fifty cross- and self-pollinated seed-berries to drop off while yet too immature for the seed to germinate. Not a single hand-pollinated flower matured its fruit.

Recently, a part of the pedigree records were lost in a fire which destroyed one of the buildings of the Connecticut Agricultural Experiment Station. For these reasons the data reported here do not represent fairly the amount of work done upon the subject, for the actual number of plants under observation was considerably larger than the figures reported. The complete figures had been studied with some care before the loss of the records, and it is thought that the remaining records are a fair sample of the whole.

The records contain observations on only one genera-

<sup>1</sup> Some essential points in potato breeding. Biennial Report, Conn. Agr. Exp. Station. 1907-1908, 429-447, 1908.



tion of plants, together with the characters possessed by their parents. I have endeavored to find what characters were possessed by the parents of the varieties used in crossing but have found no trustworthy data. The following conclusions, therefore, are tentative. Nothing is known about the behavior of the characters when extracted. The data show that *certain characters segregate*, they give some evidence as to dominance and recessiveness, but they do not show the exact behavior of the Mendelian factors concerned, under different combinations.

#### COLOR IN THE PLANT STEM

Many varieties have a purple anthocyan sap color which gives the plant stem a dark appearance quite distinct from the clear green stems of the varieties in which it is absent. The color is variable in amount in different varieties. In some it extends throughout the petioles and petiolules; in others it can only be detected on the stems of the young seedling. My counts were made on seedlings about four inches high.

The color is evidently of the same nature as that found in many other cultivated plants. Its widespread occurrence and seeming uselessness in the plant's economy would place it in the category of typical varietal characters in the sense used by De Vries. It forms a single allelomorphic pair with its absence.

One purple-stemmed variety selfed gave all purple-stemmed progeny. Four purple varieties selfed, each showed segregation into two distinct classes, purple and non-purple. Fifty-four purple plants and seventeen non-purple plants were obtained. (These figures as well as those that follow are the records saved from the fire.) In each of these cases we may take it that the parent plants were heterozygous for the purple color, and approximated the simple three to one Mendelian ratio when self-pollinated. Four green-stemmed varieties were also selfed, and produced nothing but green-stemmed progeny.

One of these pure green-stemmed varieties was crossed on one of the heterozygous purples, and thirteen seedlings were obtained. Six plants were purple-stemmed and seven were green-stemmed. This result is what would be expected when crossing DR  $\times$  R.

#### COLOR IN THE FLOWERS

All potato flowers have a ray of yellow extending from the limb of the corolla toward the apex of each lobe. The remainder of the corolla is either white or purple. There is wide zygotic (in potatoes, therefore, varietal) variation in the intensity of the purple sap color, but the flowers should probably be classed as either purple or white. The fact that the variety color, whether light or dark, remains true when propagated asexually, does not necessitate more than one Mendelian pair. It is undoubtedly a quantitative difference in the same pigment which is kept constant by the asexual method. Why somatic cell divisions should reproduce a color shade so exactly, while sexual reproduction gives rise to varying shades is unknown. It is the more peculiar since in animals visible division of the chromatin appears to be much more accurate in the sexual cells than in the somatic cells. The somatic cell appears to have the power of developing and of regenerating only the quantity of color originally apportioned to it, except on the rare occasions when all of the potential color activity goes to one daughter cell and the other is left without it. When this occurs, branches resulting from the descendants of the second cell are "sports" or "bud-variations" in which the original character is lost.

Only two varieties of potatoes with flowers other than purple or white have been noticed. The variety *Holland fleur de June* has blossoms which are decidedly yellow. Several attempts to self this variety and to cross it with other varieties failed. One other variety, a nameless seedling of unknown origin, possessed a true blue flower. No admixture of red which would give it a

purple tinge could be detected. Even this color, however, may be of the same nature as the purple color, the difference being in the completeness of the reaction forming the blue dye. It will be remembered that litmus reacts in this manner. Several cross-pollinated and several self-pollinated fruits were obtained from this variety, but none of the seedlings had flowered in 1909, the second year of their growth.

The seedlings of the potato are very slow to flower in a New England environment, and but few flower records were obtained among several hundred plants. One selfed variety with purple flowers gave progeny all with colored flowers. Three selfed varieties with purple flowers gave both purple and white flowers: the total number of seedlings that flowered was nineteen, of which fourteen were purple-flowered and five were white-flowered. Three selfed white-flowered varieties gave nothing but white-flowered progeny.

Since three white-flowered varieties gave nothing but white flowers and three out of four colored varieties showed a hybrid condition with segregation of color, the purple is probably dominant to its absence. Color and no color is probably a single Mendelian pair, but this can not be stated with certainty from such meager data.

#### COLOR IN THE TUBERS

Potato tubers, when colored,<sup>2</sup> are either purple or red. In both cases the color may extend over the entire tuber or may be limited in extent. No definite mosaic pattern is formed when the color is limited, but the splashes of color are restricted to pretty definite areas. It is probably due to a separate Mendelian factor, for the mosaic varieties and the self-colored varieties are distinct. Tuber color varies quantitatively more than flower color. Many varieties show no color in the skin, and can be classified only by examining the young shoots when the

<sup>2</sup> Colorless skins may vary from white to dark brown in different varieties. This is entirely due to their possessing corky layers of various thicknesses.

latter are about half an inch in length. The progeny of such varieties belong to the same classes as the progeny of self-colored varieties. They give fewer self-colored seedlings, however, which may be due to the action of one or more unknown heritable factors. I have not attempted to separate the self-colored from those showing color in the young shoots, but have classified both as colored varieties.

The results of selfing varieties with different color characters are as follows:

Selfed purples gave either all purples (one variety); purples, reds and colorless (two varieties); or purples and colorless (three varieties). Selfed reds gave either all reds (two varieties), or reds and colorless (two varieties). Selfed red varieties gave no purple progeny. Three colorless varieties (that is, no color in *either* the *tuber skin* or *young shoots*) were selfed, giving all colorless progeny.

Without considering factors for limiting color, these results seem to show that purple and red are separate Mendelian units, each dominant to its absence, and that purple is epistatic to red.

It is an interesting fact that although the purple varieties and the red varieties are distinct color types without intermediates and that mosaic varieties of each are known, yet in no case has a mosaic variety appeared in which splashes of the two colors are found. We may conclude therefore that the two colors are formed by the action of other factors upon the same chemical constituent. If we assume that the red color is a lower form of oxidation than the purple color and that they are produced by different oxidases R and P acting upon the same substance C, the results obtained are explained, for the presence of P would oxidize all of the substrate to the purple color.

#### SHAPE OF TUBER

Potato tubers vary in shape from a length six times the median diameter to a length about the same as the

median diameter. The varieties, the length of whose tubers is not over one and one-quarter times the medium diameter, I have called round. Two selfed round varieties gave only round progeny. Twelve varieties with oval tubers when selfed gave elongated, oval and round progeny. The ratio of other types to round was about nine to one. Either there is a series of factors for shape with the round type as the final subtraction form, or the oval types are heterozygotes of elongated and round. The latter interpretation is more likely to be correct, because oval types have been the popular market types for many years and therefore been used as parents in crosses.

#### DEPTH OF EYES

Shallow buds or eyes are required for profitable commercial varieties, yet from one fifteenth to one fourth of the progeny of ten selfed varieties were deep-eyed forms. Three selfed varieties gave no deep-eyed progeny. No progeny of deep-eyed seedlings were obtained, but it seems probable that this character is recessive to shallow eye.

The writer is fully aware that these few observations do not prove that the characters in which potato varieties differ all segregate in Mendelian proportions after crossing. A long series of crosses is necessary to analyze correctly the behavior in inheritance of such characters as shape. On the other hand, the color characters in stem, blossom and tuber are definite and discontinuous, and are alternative in inheritance. The chaotic appearance of the progeny of our commercial potatoes is only apparent. They readily fall into a simple classification and their exact behavior in inheritance could be readily determined if it were not for the difficulties attending successful crossing.

As the writer has previously stated,<sup>3</sup> certain characters pair with their own absence in crossing and these

<sup>3</sup>"The Transmission of Variations in the Potato in Asexual Reproduction," Biennial Report, Conn Agr. Exp. Sta. 1909-1910, 119-161, 1910.

character pairs are the ones affected when a somatic mutation or bud variation occurs in asexual reproduction. Simple loss of the factor takes place. Segregation, therefore, takes place at other times than the reduction of the chromosomes.

In a previous paper,<sup>4</sup> the writer analyzed the data then extant concerning the hypothesis of degeneration or "running out" of potato varieties. The conclusion was that no degeneration due to continued asexual propagation occurs. No data have been obtained which refute this view, but the study of progeny of selfed potato varieties has suggested an explanation of a certain amount of diminution in yield after long-continued asexual propagation. All commercial potato varieties which have been selfed and their progeny grown, have proved to be heterozygous in at least two characters. It has been shown<sup>5</sup> that when maize biotypes are crossed, the  $F_1$  generation has greater vigor and gives larger yields than the parents. It is a condition apart from inheritance, and is probably due to the heterozygous condition of certain characters in the germ cells. It may be correlated with the actual mechanical operation of segregation. Since potato varieties are retained in cultivation on the basis of yield and since those on the market have been found to be heterozygous in many of their characters, probably the same phenomenon is the cause. May there not be a gradual loss of the stimulus due to crossing through continued bud propagation, so that the variety has only the vigor of one homozygous in the same characters? The variety of course remains heterozygous for those characters in which it was originally heterozygous, yet there may be a gradual decline of the stimulus to cell division than it once possessed.

<sup>4</sup>A "Study of the Factors influencing the Improvement of the Potato," Bull. Ill. Agr. Exp. Sta., No. 127, 375-456, 1908.

<sup>5</sup>East, E. M., "The Distinction between Development and Heredity in Inbreeding," *AMER. NAT.*, 43: 173-181, 1909.



## SHORTER ARTICLES AND CORRESPONDENCE

### THE AGE OF SPEED SIRES

IN the May number of the *NATURALIST*, Mr. Redfield makes reply to the criticism of his theory which I made in the issue of January of last year.

Recalling the interest evidenced by biologists when Mr. Redfield's theory appeared some years ago, and considering that no one else has criticized his figures, I assume the question to still be a proper one for discussion.

Mr. Redfield's conception of acquired dynamic development and the data he presented to show its inheritance, strongly suggests the direct transmission of effects of use of the organs.

It is true that more than a majority of successful breeders of trotting horses believe the results of use to be transmitted. A settlement of the question is of no direct interest to horse breeding interests. A change of opinion would not change their practise. Selection and environment are the fundamental factors upon which their work is based. It is immaterial whether they consider one or the other to be of greatest moment; both are imperative. Mr. Redfield would say that the effect of the environment is transmitted. Selectionists would say that the racing test as a feature of environment is an indispensable aid to selection of good individuals and is the only real proof of individual merit.

The fact that a few breeders who regard training as an aid to selection have been as successful as any of the breeders who think otherwise, goes to show that in the breeding of trotters, practises of best breeders vary but little. They differ in their explanation of how the two factors exert their influence upon the results. This lack of agreement, while of no immediate import in practical breeding, because it bears upon a principle involved is of primary interest in the scientific study of heredity.

In my contribution last year, I criticized Mr. Redfield's figures only as they related to the age of sires. He assumes dynamic development to be proportionate to the amount of racing and to the age.

Mr. Redfield's own words will explain the evidence from which he argues for the value of age:

I said that I took one thousand registered stallions, alphabetically, from the "Index Digest" of the "Register," and calculated the ages of their sires at the time when these registered stallions were foaled. From these I determined that the average time between generations in the male line was 10.43 years, which would give the average age of sires as 9.43 years at the time of service. I then said that, making all reasonable allowances for errors, the average time between generations in the male line might be set down as between 10 and 11 years, and that this period might be used as a standard in testing the age part of the theory. So far no one claims to have tested the accuracy of my calculation; no one claims that the figures I gave were wrong; and no one has said that these figures can not properly be used as a standard; yet if I am to be controverted, one of the first things to be done is to dispute the accuracy of my standard.

I then took the entire list of 2.10 trotters as an appropriate class of animals to be used in testing the inheritance of dynamic development, and I calculated the ages of their male progenitors for four generations. The number of animals involved was over five thousand and I gave the average time between generations in the male line for the production of 2.10 trotters as being approximately 14.00 years. This is an average of nearly 40 per cent. over the standard average determined from the "Register," and my explanation of this remarkable difference was that it indicated the inheritance of acquired dynamic development. So far no one has disputed the accuracy of my computation and no one has attempted to give any other explanation of such an unusual divergence from the natural order of things.

My objection was to comparing the average age of immediate sires in one case with that of all sires in four generations in the other case. I showed that when we take only immediate sires in both cases, it is shown that the average age of the sires of 2.10 horses is practically the same as that of average horses as given by Mr. Redfield.

He now shows that in the case of the 2.10 trotters, while their sires were of an average age of 10.4 years, their grandsires averaged 12.5 years, the great-grandsires 13.5 years, the great-great-grandsires 14.5 years; the stallions appearing in the next two lines at the ages of 15 and 15.98 years.

The evident conclusion from this statement is that our best horses have come from an increasing popularity of *younger* sires.

But this statement regarding the age of sires in various lines

is used in contrast with conditions in average horses and the actual figures for the ages of progenitors of average horses are not given to us, but assumed to be much lower. This assumption is erroneous.

It would be very interesting to have the average ages of grandsires, great-grandsires, etc., of the first thousand horses named in the "Index Digest" and used by Mr. Redfield to represent average horses.

I have considered it fairer, however, to use a group, though smaller, more nearly contemporaneous with the 242 horses of 2.10 records used in my previous study. It would be desirable to have a figure based upon the study of the 2.10 list as it stood at the end of 1909, but a comparison of the two groups here used is, I think, a fair one. The group used to represent average horses and as having been bred at about the same time as the 242<sup>1</sup> horses with 2.10 records consists of the first 242 horses registered in Volume 15 of the "Register."

The following tabulation will show that the two groups were contemporaneous:

	Foaled before 1880.	Foaled 1880-1885.	Foaled 1886-1890.	Foaled 1891-1895.	Foaled 1896-1900.
2.10 horses	2	12	40	95	81
Average horses from Vol. 15	2	3	14	116	107

The average age of stallions appearing in each line of the pedigree of the above is as follows:

AVERAGE AGE OF

	Sires.	Grandsires.	Great Grandsires.	Great Great Grandsires.
2.10 horses	9.4 <sup>2</sup>	11.5 <sup>2</sup>	12.5 <sup>2</sup>	13.5 <sup>2</sup>
Average horses from Vol. 15	8.28	10.65	11.64	12.78

I am still of the opinion that an impartial study of the figures does not show that age is, of itself, any factor in the inheritance of speed.

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<sup>1</sup> Vol. 22 of the "Year Book" gives 279 fast (2.10) horses, but it is possible to determine the age of the sires of only 242 of these.

<sup>2</sup> One year less than given by Mr. Redfield to show the age of stallions at the time the foals were sired.

## NOTES AND LITERATURE

### RUSSO ON SEX-DETERMINATION AND ARTIFICIAL MODIFICATION OF THE MENDELIAN RATIOS

IN the April number of the *NATURALIST* Professor Jordan presents an interesting review of recent literature on sex-determination in the course of which he gives an extended account of Russo's<sup>1</sup> experiments with lecithin-fed rabbits. Great importance is attached to this work because it points to conclusions diametrically opposed to those reached within the last ten years by nearly every one else who has studied sex-determination either from the standpoint of the cytologist or from that of the experimental breeder. The nearly unanimous verdict has been that sex-determination is a matter of gametic differentiation, and that the sex of a developing organism is not influenced by conditions of nutrition either applied to it directly or brought to bear upon the mother. The only exception, apparent but not real, as I pointed out in 1903,<sup>2</sup> is afforded by organisms in which both parthenogenesis and sexual reproduction occur. Abundant nutrition favors parthenogenesis, scanty nutrition causes a return to sexual reproduction, including the production of males. Russo has revived the older idea that in non-parthenogenetic organisms also scanty nutrition is a cause of male-production. If true, this is a matter of the greatest importance, both theoretical and practical. Jordan is quite right in so regarding it, but has apparently failed to appreciate the uncritical character of Russo's evidence. This has, however, already been pointed out by several writers, most recently by Punnett,<sup>3</sup> whose brief but convincing paper seems not to have been seen by Jordan. Russo himself has never published the results of his experiments as a whole, but only of *selected experiments* the results of which were favorable to his thesis. This fact alone would throw Russo's claim out of an impartial court, but to be more than fair to Russo, it may be said that his experiments have been

<sup>1</sup> Russo, A., "Studien über die Bestimmung des weiblichen Geschlechtes," G. Fischer, Jena, 1909.

<sup>2</sup> Castle, W. E., *Bull. Mus. Comp. Zool.*

<sup>3</sup> Punnett, R. C., *Proc. Cambridge Phil. Soc.*, Vol. 15, Pt. 2, p. 92, 1909.

repeated twice independently, by Basile<sup>4</sup> and by Punnett, both times with negative results.

Russo's original evidence consisted of 100 *selected* litters of young borne by lecithin-treated does which included 217 male and 431 female rabbits, and of 100 control litters (likewise *selected*) from non-treated does which included 400 male and 287 female rabbits. The claim is made that lecithin treatment raised the percentage of female offspring from 41.8 to 56.5. Russo in this case proves too much, for his selected controls fall as much below the normal percentage of females as his selected lecithin-treated cases exceed it and, it is fair to assume, for the same reason, because they are *selected*. It is astonishing that a scientist should present such evidence. A prosecuting attorney who should be allowed to decide what evidence should be presented to the court, both for and against, and that all else should be excluded might convict any one of us of all the crimes enumerated in the criminal code.

On the other hand, Basile and Punnett give *all* their results, not selected ones; they mean to tell the *whole* truth, not selected truth. Basile obtained from lecithin-treated does 66 male and 51 female young, or 43.6 per cent. females, while from controls he obtained 225 male and 215 female young, or 48.8 per cent. females. This result is contrary in nature to that of Russo. If any effect is to be ascribed to the lecithin, it is in this case male-production rather than female-production as claimed by Russo. Punnett in repeating Russo's experiments followed methods outlined for him in detail by Russo. They are therefore particularly satisfactory. From lecithin-fed does he obtained 24 male and 23 female young, or 48.9 per cent. females; from controls 54 male and 49 female young, or 47.6 per cent. females. These proportions agree closely with those obtained by Basile, and accord with Hurst's extensive observations cited by Bateson<sup>5</sup> to the effect that "under normal conditions male and female birth are sensibly equal," in the case of rabbits. Finally Heape<sup>6</sup> has criticized on cytological grounds the supposed histological distinction between male and female ova of the rabbit as described by Russo. Heape shows that in all probability the fat-containing, supposedly female ova are degenerating ova.

<sup>4</sup> Basile, C., *Atti Acad. Lincei*, Vol. 17, p. 643, 1908.

<sup>5</sup> Bateson, W., "Mendel's Principles of Heredity," 1909.

<sup>6</sup> Heape, W., *Proc. Cambridge Phil. Soc.*, Vol. 14, p. 609.

If further evidence were wanting of the wholly uncritical character of Russo's work, it might be found in his analysis of the results of crosses involving Mendelian color characters. Jordan correctly states Russo's claim, though not his evidence. The claim is that when albino or spotted females were mated with gray or black males, the color character of the latter dominated in the young, but when the same females were first treated with lecithin and then mated with a black male the color character of the mother occurred in part or all of the young. The conclusion is drawn that lecithin treatment of the mother is a practical means of fixing a desired maternal character as a racial character, a claim so astonishing and so important, if true, that with Jordan's indorsement it is in danger of being taken seriously by readers of the NATURALIST. For that reason I have been prompted to examine Russo's evidence carefully and to write this note. His evidence consists of four cases, *all* the cases bearing on this point concerning which he gives detailed information, and he states that these are his *best* cases.

*Case 1.*—A young albino (Polish) female was treated with lecithin and mated with a gray male and produced two gray young. The lecithin treatment was continued and she was now mated with a black male and had eight young, six white and two black. The reader is led to conclude that the *further* lecithin treatment is responsible for the appearance of white young in the second litter. But is it? No evidence is given as to the gametic character of the male parent of either litter. If the gray male was homozygous he should have produced, as observed, only gray young, though the production of two young only was no adequate test of his character. The *same* male should have been used after the "further" lecithin treatment if information were desired about the effect of lecithin, but this was not done. A new untried male of a different color, black, was used. He produced both albino and black young, a result which indicates that he was heterozygous. If so, the result obtained is such as would have followed without any lecithin treatment whatever! The case proves absolutely nothing, because neither colored parent was tested, so far as we have any information. Russo seems to think that the first litter affords a criterion of what the second should give by a different male parent, a conclusion which no one familiar with Mendelian principles would entertain for a moment.



*Case 2.*—A young Himalayan (albino) female rabbit was treated with lecithin and mated with a black male. She produced by him three litters of young. The first litter contained six young, all black. The second litter contained four young, two of which were Himalayan, two yellow-and-white.<sup>7</sup> The third litter contained seven young, concerning the color of one of which no account is given; four were Himalayan, one yellow-and-white and one black-and-white. The case (a selected one) is supposed to show that continued treatment with lecithin produces more young of the maternal type in later than in earlier litters. But a *selected case* can not fairly be used to show anything of the sort. Only a fairly uniform result of repeated experiments could establish it. Under the circumstances we can only ask what the expected Mendelian result would be in a case like this if no lecithin treatment were involved, and then inquire whether the observed result differs in any respect from it.

If no lecithin treatment were involved we should say that the black male used was heterozygous in the recessive characters albinism, spotting and yellow; and the expectation would be that he would produce young of five sorts in one of the following distributions, depending on the gametic character of his albino mate:

Black		Yellow		Black and white		Yellow and white		Albino
1	:	1	:	1	:	1	:	4
3	:	3	:	1	:	1	:	8
9	:	3	:	3	:	1	:	16

The observed distribution for the three litters is 6:0:1:3:6. This is so good a Mendelian result that it would not call for any special comment in ordinary breeding work. Further matings would be called for, but it would be surprising if these did not supply the missing class, yellow, and give numbers agreeing more closely with one or another of the suggested ratios.

No control matings of either parent in case 2 are recorded by Russo.

<sup>7</sup>Regarding this class Russo says they were of the same race as the mother but with yellowish or white ground-color ("von derselben Rasse, aber mit gelblicher bzw. weisser (wie in Fig. 27), Grundfarbe"). But the photograph, Fig. 27, shows unmistakably that these animals were not albinos at all but Dutch-marked colored animals, which invariably have dark eyes. A spotted dark-eyed animal is not by any means of the "same race" as an albino, as is well known to students of genetics. Spotting behaves as a Mendelian unit-character wholly independent of albinism.

*Case 3.*—The statements of Russo concerning this case are very incomplete. A spotted "Olandese" female after lecithin treatment was mated with a silver-gray male and produced three spotted and one self-colored young, which are figured. Before treatment with lecithin she had been mated under normal conditions (*once* with the same silver-gray male, "mit demselben grau Männchen einmal," but we are not told that he was the father of the young, we are rather left to infer that she may have mated with other males also. Five young were afterward born—"es erzeugte dann später 5 Junge," all gray. Again the female was mated under similar uncertain conditions, "unter den gleichen Bedingungen," and produced four gray young. Since in a third litter, after lecithin treatment, she produced spotted young, it is concluded by Russo that the lecithin treatment was responsible for the changed result. But before accepting this conclusion we should like to be assured that one and the same male was used in both cases. The silver male may have been heterozygous in spotting, the father of the earlier litters not.

*Case 4.*—Two Himalayan (albino) females, sisters, were mated with the same black male. One of the two was lecithin treated, the other was not. Both produced black young, five each. This looks like a bad case to select in support of lecithin influence, but we are told further that in the following year, when lecithin injections were employed forty days previous to copulation, a litter of two Himalayan young was obtained. The sire of the litter is figured but we are not told whether he was the *same* as the one used the previous season. This omission is significant. The male figured was evidently heterozygous in albinism, but it is a pretty safe guess that he was not identical with the male used the previous season, which was in all probability homozygous in black. In this case as in the others described by Russo it is impossible to avoid the impression that the author is not making a full and frank statement of facts but is stating half-truths likely to mislead the unwary. In Jordan's case this is exactly what has happened. He says: "The evidence here appears unequivocal that external conditions (*e. g.*, nutrition) can determine the kind of sex and entirely vitiate the Mendelian scheme of ordinary crosses" [*italics mine*]. I have, however, presented in detail *all the evidence* which Russo gives in support of the assumed vitiation of Mendelian inheritance. I think the reader will agree with me that the evidence

is more than equivocal and that the vitiation is of reasoning not of inheritance.

So far as I know, only one other investigator besides Russo has laid claim to having modified the ordinary course of Mendelian inheritance by external conditions. Such a claim, if I rightly understood him, was made by Professor W. L. Tower, in a paper read at the last annual meeting of the American Society of Naturalists, in the case of beetles of the genus *Leptinotarsa*. Detailed information regarding the cases in question will be awaited with much interest. It is to be hoped that this will prove more complete and satisfactory than Russo's.

W. E. CASTLE.

HARVARD UNIVERSITY,  
June 11, 1910.

### THE BUBONIC PLAGUE

BUBONIC plague is primarily an animal disease. Its original victim is said to have been a species of rodents found in the mountains of Mongolia. Several Russian scientists have sought to establish this hypothesis, according to which the *Arctomys bobac*, a Mongolian marmot, is the primitive animal host. In it the disease is permanently prevalent, and from it both man and the rat are infected periodically. According to the view of these writers the final eradication of the plague from our globe would be accomplished by the extermination of this rodent. That this view is over-optimistic may be inferred from the probable existence of other ancient centers from which plague epidemics have originated, and in which, consequently, permanently infected animals are at home, and also from the recent origin of such an established center of animal infection on our own continent.

In the introduction to an article on Plague Eradication Measures (Squirrel Campaign) in California, Rucker<sup>1</sup> comments on the epizootic which for four years has been spreading among the ground squirrels of Contra Costa County, and which more recently has been reported from other districts also. The animal infected is *Citellus beecheyi*, which is reported by ranchers to have died by the thousands in 1904-5-6. In appearance and habits, it resembles closely the Thibetan marmot referred to above. Not until there came cases of human plague due to plague-infected squirrels was the disease of the latter subject

<sup>1</sup> *Journal Amer. Med. Assn.*, Vol. 53, p. 1995.

to careful scrutiny. Wherry<sup>2</sup> first showed in a conclusive manner that ground squirrels, obtained from two widely separated sections of California, were infected with the bacillus of bubonic plague. There remained to be ascertained the extent of the infection and appropriate methods for the extermination of the infected animals and the eradication of the disease, which had gained apparently a permanent foothold on the state of California. The records of these investigations form one of the most interesting and important chapters of medical zoology yet written.

The splendid work which has been done by the Public Health and Marine Hospital Service on the Pacific coast in connection with the problem of stamping out bubonic plague, has included investigations on the rats and also on native rodents, which have established important points in the relations of these animals to the spread of that disease. While no cases of human plague have been reported for many months, yet plague-infected rodents have been killed at one point or another as recently as February. The disease is not common, since about 2,000 squirrels from one county were examined before an infected individual was found. Nevertheless, one can not doubt its continuance among the wild rodents, or question the advisability of prosecuting the campaign for the total eradication of the infected animals. The service has published<sup>3</sup> complete statistics of work to date and a map showing the area studied and the prevalence of plague among ground squirrels.

The rodents in which plague infection has been demonstrated include both the introduced species, *Mus rattus*, the black rat, and *Mus norvegicus*, the brown rat, and also the native species, *Citellus beecheyi*, the California ground squirrel, and *Neotoma fuscipes*, the brush rat. The work of McCoy<sup>4</sup> corroborated fully the findings of Wherry, and left no doubt that the disease among ground squirrels is due to the same organism that causes bubonic plague among rats and men. The experimental evidence which the latter reports in a conservative and critical manner includes several typical cases of plague in human beings where the diagnosis has been verified by bacteriological methods, and where the cases "have been traced to squirrel infection as clearly as one can trace such things." While McCoy states that in his

<sup>2</sup> *Journal of Infectious Diseases*, Vol. 5, p. 485.

<sup>3</sup> "Public Health Reports," Vol. 25, p. 585.

<sup>4</sup> "Public Health Reports," Vol. 25, pp. 27-33.

opinion the number of human beings infected directly by squirrels will never constitute a large element in the infected region, yet, on the other hand, one can not deny the patent facts that the infection of native rodents provides a retreat in which the disease is relatively safe from elimination and also a source from which it may be at any time transmitted anew to the human species. The transmission from rat to man through the intermediation of the fleas is easily conceived. The intimate association of the rat with human dwellings and with places constantly visited by man makes the transfer of the infected fleas an easy matter. The transmission of the disease in similar fashion among the animals in a squirrel colony is equally readily understood, though the booby owl, which regularly occupies the same burrows with the ground squirrel, may play the important part in the dissemination of the disease, since the bird, flying from burrow to burrow, might readily carry infected fleas over long distances. If this be true, the eradication of the disease is greatly complicated.<sup>5</sup> The intimate association of rats and ground squirrels has been observed repeatedly. In one locality in the outskirts of a city both were taken alive from the same burrows and rat fleas were combed from the hair of the squirrel.<sup>6</sup>

The mode of transfer from squirrel to man is more difficult to understand. Simpson<sup>7</sup> suggests that cattle on the range are the unrecognized factor which provides for the conveyance of infected fleas from squirrel to man. He states that fleas abound in and about squirrel villages and the cattle as they range over this territory lie down to rest in and among these villages. Since the fleas quickly desert an animal after death, the cattle will more readily acquire fleas in villages containing infected squirrels, and especially if dairy cattle were concerned, the daily contact with men would give abundant opportunity for the transfer of the infected fleas. Some of the squirrel villages known to be plague infected are so isolated as to afford only occasional contact with man, yet cattle were seen grazing near these villages and may furnish the connecting link in transmission.

Of the species of wild rodents known to be infected in nature, the California ground squirrel, *Citellus beechyi*, is unquestionably the most important. It has also been the longest recognized

<sup>5</sup>Rucker, W. C., "Public Health Reports," Vol. 24, p. 1225.

<sup>6</sup>"Public Health Reports," Vol. 25, p. 623.

<sup>7</sup>"Public Health Reports," Vol. 25, p. 250.

as an element in the plague situation. Its habits and distribution have been outlined by Merriam,<sup>8</sup> who also discusses means for its systematic destruction.

Much less well known is another California rodent which has very recently been shown to be susceptible to bubonic plague under natural conditions. This species is the woodrat, *Neotoma fuscipes annectens* Eliot. It is distinctly a new world form, of which several species occur on the Pacific coast and into the desert region as far as Utah and Colorado. Evidently, if the infection can be transmitted from one to another of these species, the disease will thus extend over a large area. Rucker,<sup>9</sup> says of their habits:

Wood rats are nocturnal in their habits and are seldom seen in the light of open day except when it is very cloudy. For the most part, they are found along small wooded arroyos, in which they build their nests, often of the most elaborate design. Those which the writer has had the opportunity of dissecting consist of pieces of driftwood arranged in a pile, sometimes 6 or 7 feet in diameter and 3 feet high. There is usually only one entrance to the nest and this is lined with sharp sticks. It is said that where cactus is plentiful the tunnel is lined with cactus spines as a protective measure against other mammals. The interior of the nest is frequently arranged into three stories, and contains storehouses and living rooms. Usually there is an exit which is frequently found near the base of a tree. This is utilized as a means of escape when the ordinary entrance is blocked and some enemy begins to tear the nest apart. The storehouses in several instances contained large quantities of the corms of a plant growing in the immediate neighborhood. Although wheat was growing but a few hundred yards away, none of this was found in these nests. In certain regions the *Neotoma* store up large quantities of mesquite beans, and these caches are raided annually by the Indians, who use them for food. They also store up mushrooms, certain varieties of puffballs and acorns.

In view of the part played by fleas in the transmission of the disease, it is interesting to note the average number of fleas from a squirrel is much larger than from a rat or from any other host yet observed. Much collateral work has been done on the species of fleas, found on the various rodents which suffer from bubonic plague, and on the relations of these fleas to the transfer of the disease as shown by their ability or readiness to bite man and other hosts. The majority of the two rat fleas common in San Francisco, viz., *Læmopsylla cheopis* Roth and *Ceratophyllus*

<sup>8</sup>"Public Health Reports," Vol. 23, No. 52.

<sup>9</sup>"Public Health Reports," Vol. 25, No. 1, p. 2.



*fasciatus* Bosc., will bite man under experimental conditions, while the squirrel fleas, *Hoplopsyllus anomalus* and *Ceratophyllus acutus*, feed readily on man's blood.<sup>10</sup> The same authors have also shown<sup>11</sup> that fleas from rodents will adapt themselves to a host of a different species and that fleas from squirrels will attack rats even in the presence of their normal host. Plague bacilli have been demonstrated in both the common squirrel flea, *Ceratophyllus acutus*, and also in the lice (probably *Hæmatopinus montanus*) very commonly found on the same host.<sup>12</sup> Experimental work has also been done to determine the susceptibility to bubonic plague of other rodents in which the disease has not yet been reported under natural conditions.<sup>13</sup> Finally in experimental cases<sup>14</sup> rat fleas have conveyed plague from rats to ground squirrels and squirrel fleas from squirrel to squirrel, and also to guinea pigs and rats.

H. B. WARD.

#### DESERT PLANTS<sup>1</sup>

In the opinion of the reviewer, this book constitutes the most noteworthy contribution thus far submitted from the Desert Botanical Laboratory. In a measure it may be said to be the outcome of previous contributions and others not hitherto published. Investigations in progress there during the several years since the establishment of the desert laboratory, while they have a wider range of application, have centered in the effort to describe and interpret the interplay of stimulus and response as between desert environment and plants in the desert. It in no way detracts from the merit of Professor Spalding's contribution to say that the results of these investigations constitute a prominent feature of the book, and indeed it stimulates increased appreciation of his work to observe that he assumed the difficult task of so correlating the results of a staff of specialist investigators as to bring about a reasonable measure of interpretation of the rôle of environmental stimuli in shaping the origin, distribution, associations and movements of desert plants. The book lays claim to being only a partial interpretation of the

<sup>10</sup> McCoy and Mitzmain, "Public Health Reports," Vol. 24, No. 8.

<sup>11</sup> "Public Health Reports," Vol. 24, p. 1013.

<sup>12</sup> Geo. W. McCoy, "Public Health Reports," Vol. 24, p. 475.

<sup>13</sup> Geo. W. McCoy, *Jour Infectious Diseases*, Vol. 6, p. 283.

<sup>14</sup> Public Health Reports, Vol. 25, p. 465 and 659.

<sup>1</sup> "Distribution and Movements of Desert Plants," by Volney M. Spalding, Carnegie Institution of Washington, publication No. 113, October, 1909, pp. 144, with 30 plates.

problems of desert vegetation and expressly points out the need of continued investigation not merely of the area under consideration (the so-called "Desert Laboratory domain"), but of comparative studies of more areas widely distributed through the desert.

Having set itself the task of interpreting the ways of plants in the desert, the management of the desert laboratory began by investigating intensively a small area of typical desert country. The laboratory site appears to have been selected with a view to having at its door this ideal field of investigation, which is an area of not over a mile in radius.

Professor Spalding rightly supposes that no other small area anywhere has had centered upon it the intensive study of so many well-equipped investigators as has this typical bit of desert, and he might have added that perhaps in no other case has there been a more adequate equipment of apparatus and instruments for work of a precise nature and, in the reviewer's opinion, no other instance where more adequate methods have been devised for pursuing the investigations.

The work of collaborating specialists is largely presented as distinct sections or chapters of the book.

The geology of the vicinity of the Tumamoc Hills is presented with particular reference to its relation to the distribution and movements of plants by Professor C. F. Tolman, of the University of Arizona (pp. 67-82), in the chapter on Environmental and Historical Factors. In this chapter also Dr. B. E. Livingston contributes a section (pp. 83-93) on the soils of the desert laboratory domain. Reference is made here likewise to the evaporation studies conducted by Dr. Livingston. It is of course beyond the range of the present reviewer's task to take up Dr. Livingston's work in detail, but it seems an appropriate place in which to point out a striking case in which endowed research has yielded results of far-reaching application in behalf of economic development, namely, in the relation of soils and evaporation to plant production generally.

Dr. W. A. Cannon, of the Desert Laboratory staff, contributes a thoroughgoing study of the root system of the giant cactus (*Cereus giganteus*), together with a comparison of the root systems of certain other marked desert species (pp. 59-66). Such specific studies furnish the source of reliable data for conclusions as to the relation which the habits and structure of plants bear to their distribution.

Floristic investigations have been undertaken by Professor J. J. Thornbur, botanist of the Arizona Experiment Station, and reported in an effective manner under the title *Vegetation Groups of the Desert Laboratory Domain* (Chap. IV, pp. 103-112). In this contribution, floristic studies, instead of being dry lists of species, are made to contribute materially to the chief questions as to the origin, association and movements of the vegetation.

In the chapter devoted to defining plant associations and habitats, pages 24 to 27 embrace an account of the lichens of the Laboratory domain, by Professor Bruce Fink, of Miami University.

Distributional maps with accompanying notes were prepared by Mr. J. C. Blumer, of the laboratory staff. In the reviewer's opinion maps of this sort, showing, as they do, location and individual frequency of notable species, are of essential help to the reader, as they must have been indispensable to the writer of the book.

Finally, among the collaborative contributions is a chapter (V, pp. 113-119) on the *Origin of Desert Floras*, by Dr. D. T. MacDougal, director of the Desert Laboratory and of the Division of Botanical Research of the Carnegie Institution of Washington.

In this study of the vegetation of the Desert Laboratory domain Professor Spalding has led the attack from the plant side, seeking to define the actual conditions of distribution, association and movements in the vegetation as a whole and specifically in the case of prominent desert species, and always with a view to relating facts of distribution, structure, etc., with the environmental conditions under which they have been worked out.

Accordingly, a considerable portion of the book is of a descriptive nature. Chapter I presents the plant associations and habitats of the laboratory domain. The author distinguishes some twelve associations exclusive of parasitic and symbiotic plants and miscellaneous introduced species, meaning by "association" an aspect or phase of vegetation, such, for example, as the cottonwoods and willows of a river bank, a mesquite forest, or chaparral made up chiefly of palo verde and catclaw or, on the other hand, of Mexican greasewood. These associations reflect topographic and soil conditions of the area. Remembering the traditional view as to the hostility of the desert toward plants, one is rather surprised to learn that this inhospitable domain

harbors between four and five hundred species which are by no means all xerophytes, but species adjusted to the most diverse moisture requirements. To be sure, this is due in a measure to the inclusion of a river and its flood plain in the Laboratory domain. Still, a surprising number of species manage to exist on the arid situations without being marked by any special xerophytic structures.

Chapter II takes up in detail an analysis of the distribution of certain of the most prominent desert species. As to the factors determining the local distribution and association of species, these are, just as everywhere else, chiefly topographic and soil conditions. Manifestly it is primarily the water supply as affected by conditions of soil and topography which is the critical factor, but soil drainage or aeration is found to play a large rôle and, indeed, is the determining factor in the distribution of certain species.

Special attention may be called to that portion of Chapter II which presents the facts and conclusions as to aspect preference as exhibited on the laboratory domain. The question, in brief, is to account for the differences in the vegetation covering of slopes having a general southerly exposure as compared with those having a general northerly one. Here is found fully justified the claim advanced by certain American plant ecologists, that only by exact quantitative study of the items involved may one expect to arrive at an explanation of distribution, association and movements of plants. In this connection the study of the root system of *Cereus giganteus* sustains its status as a model piece of ecological investigation.

Another thing which we learn from this chapter (II) is that while, as we supposed, the chief struggle of plants in the desert is against the physical environment, there is still a right marked element of competition amongst desert species with its consequent features of accommodation (commensalism?) or of extermination and succession. Since, however, the problems of distribution, association and movements are so little complicated by the factor of crowding as compared with mesophytic forest societies, for example, it has seemed to the reviewer that the desert furnishes about the simplest as well as the most inviting point of attack for the ecologist.

In chapter VI under Review and Discussion and in Chapter VII, a summary, Professor Spalding brings forward in condensed form a survey of the work of investigation done on the

laboratory domain and particularly the tentative or positive conclusions he is able to formulate as a result of these investigations. So far as regards the local distribution and association of species, the reviewer is unable to discern that the general view thus far held as to the rôle of the so-called edaphic factors is in any wise modified, but our knowledge of the manner in which this rôle is played has been materially enriched by the work of Professor Spalding and his collaborators.

Foregoing the temptation to comment on many other interesting details of this book, there remains yet to consider the crowning question as to whence came the desert vegetation and how did it come to be what it is? Upon this point Professor Spalding's conclusions are quite positive, as indicated in the following words from his book:

The general continuity of geological history since the Tertiary indicates a relatively long period within which plants of the Laboratory domain have one by one or at any rate by no mass movements, become established in their places. There is reason to believe that throughout this period the processes now going on before our eyes have been in progress. The present flora, therefore, may be assumed to be merely the final stage so far of just such a series of events as are now observable. . . . The small area within its limits (the Laboratory domain) has received representatives of genera that have shared in the great migrations south and north along the Cordilleras, but through the time that has elapsed since the greater movements it has also received by entirely ordinary means the plants that have come and are still coming to it.

We are unable to gather from these words whether Professor Spalding means that "the processes going on before our eyes" relates simply to movements of vegetation or includes also modifications whereby ecological types—as, for example, the giant cactus—arise. In this connection, Dr. MacDougal's chapter on the origin of desert floras is illuminating. His view is in agreement with Professor Spalding's as to the fact that there is little evidence of extensive migrations of the characteristic xerophytes of this desert from which it is to be inferred that these investigators believe the Sonoran desert to have been and to be now a center of origins for its flora. MacDougal brings us face to face with the question as to *how the desert makes its flora* if one may give the question this popular cast. He rejects the idea of so-called adaptive changes representing ontogenetic and morphogenetic responses (made by a plant when exposed to desert con-



ditions, for example) as being not necessarily adaptive and in any event not capable of being transmitted to offspring. He believes, however, that what has been observed to take place under experimental conditions might take place when plants or generations of plants are subjected to the stimulus of desert environment. His words are:

The influence of external conditions upon the germ plasm, however, has been seen to produce irreversible changes in a hereditary line by which new combinations of qualities and new characters were called out, which were fully transmissible. Furthermore the newly produced forms perished in some localities endured by the parental type, but exceeded it in weathering the conditions of other localities.

In the treatment of the larger aspects of the question of the origin and movements of desert plants the intensive study of a single small area fails to furnish a large enough fund of comparative data to make a wholly satisfactory basis for conclusions. The reviewer would modestly suggest that the Sonoran desert (meaning the desert areas of our southwest and of northern Mexico) is a very large and diverse region in which not merely individual species but whole genetic groups have become xerophilized (with apologies for spurious coinage), and that correlated investigations at numerous points and a study of genetic relationships in connection with distribution, association, movements and modification (meaning transmissible qualities or characters) would furnish a broader basis for the interpretation of the ways of plants in the desert at large.

In a final word the reviewer would invite attention to the status of Professor Spalding's book considered in relation to plant ecology. In full appreciation of other recent ecological contributions of similar merit the opinion is advanced that this book stands in a special way as an index of the new era in geographic ecology in which a field of botanical research which was prone to abound in verbosity and in the discovery of "adaptations" has been brought to the status of a more exact science employing quantitative methods of study. It is not boastful to say that this newer phase of plant ecology represents especially the outcome of the teaching and investigations of a small group of American botanists (of which group the present reviewer can not claim to be a member) who have sought to put the study of plant relations upon the same basis as that pursued in the study of plant physiology in the limited sense.

WILLIAM L. BRAY.



